THE FALLACY

OF THE

PRESENT THEORY OF SOUND

MOTT.

J. M. D. Potts

April 1885

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PRESENT THEORY OF SOUND

BY

HENRY A. MOTT, JR., PH.D., E. M., ETC.

PROFESSOR OF CHEMISTRY AND PHYSICS IN THE NEW YORK MEDICAL COLLEGE AND HOSPITAL FOR WOMEN

AUTHOR OF

"*The Chemist's Manual*"; "*Was Man Created?*"; "*Adulteration of Milk*"; "*Testing the Value of Rifles by Firing under Water*"; "*The Laws of Nature*"; "*The Air We Breathe, and Ventilations*," etc.

NEW YORK

PRINTED FOR THE AUTHOR BY

JOHN WILEY & SONS

15 ASTOR PLACE

1885

M. H. GREEN,
74 and 76 Beekman Street,
NEW YORK.

PREFACE.

To attack a theory which has been upheld for 2500 years, and which has been and is sustained by the greatest living scientists, is certainly a very bold undertaking. The nature of the undertaking should not, however, deter truth from asserting itself, or deter any scientific man, with sufficient individuality and independence, from exposing the fallacy of the same, if such fallacy can be shown to exist.

The author therefore makes no apology for thus coming to the front and joining Dr. A. Wilford Hall in exposing the fallacy of the present theory of sound, for he believes that it is the duty of every man to do his own thinking and not allow others to think for him. The old saying,

> "The name is but the shadow, which we find
> Too often larger than the man behind,"

is too true to be overlooked. Great names often carry with them too much authority, and it requires a well-balanced mind to properly attach the correct importance which in

many cases such names deservedly merit. To accept, therefore, the present theory of sound as correct because it is sustained by "great names," and because on the authority of such names it is pronounced correct, without exercising any individuality of thought to inquire into its merits or demerits, would be only to impede the progress in search of truth, and not only impair the value of scientific deductions, but at the same time limit the progress of science.

That the reader may feel satisfied that other scientific men besides those mentioned in the following pages have arrived at the same conclusions as the author, he deems it advisable to introduce here the opinions of some well-known scientists on Dr. Hall's discovery.

Opinion of Dr. H. H. Adams, President of Wesleyan University.

"I have examined the new theory of sound—or rather, as it seems to me, *the complete overthrow of the undulatory theory.* Other members of the faculty have come to the same conclusion as myself."

Opinion of Prof. J. L. Kephart, A.M., of San Joaquin College, Cal. (formerly of Western College, Iowa).

"I have no hesitation in admitting that, in my opinion, the undulatory theory of sound is hopelessly shattered."

Opinion of Prof. Osborn, LL.D., of Madison University.

"The part on sound I prize very highly—a new departure that must be *permanent* and lead to many modifications of old notions."

Opinion of Henry C. Cox, A.M., Professor of Physical Sciences, Pickard Institute, Chicago.

"The first division of the book is given to a discussion of the wave-theory of sound; and so completely does he show the absurdity of that hypothesis that we feel mortified to reflect that for fifteen years we taught it as science."

Prof. Thomas Munnell, A.M., former President of Hiram College, Ohio, no longer believes in the current theory of sound, and thinks it will not be long before the scientific leaders of this century will abandon it also.

Prof. J. W. Spangler, President of the American College, Concepcion, Chili, S. A., states that "the wave-theory of sound has been demonstrated [by Dr. Hall] to be a pure fallacy."

Prof. Jacob Chapman, A.M., of Exeter, N. H., formerly Professor of Mathematics in Dartmouth College, and who held the same position for years in the Franklin

and Marshall College, and who believed in and taught the wave-theory of sound, has declared that he has abandoned that theory as entirely wrong.

Charles H. Goddard, B.A., LL.B., Professor of Physical Sciences and Biology in Nebraska College, at Nebraska City, has abandoned the wave-theory of sound as false.

Prof. J. W. Lowber, M.A., Ph.D., former President of Columbia College, Ky., writes that he has totally abandoned the wave or undulatory theory of sound.

Prof. C. H. Kiracofe, A.M., President of Hartsville (Indiana) University, states: "We no longer teach the wave-theory of sound as science, but as a theory worthy of consideration only as an example of what may be palmed off on the world as true science."

It must be agreed that the opinions here presented against the wave-theory of sound cannot be ignored as unworthy of notice by the eminent scientific men assailed; for such an array of careful thinkers as above (*and numerous others could be mentioned*) who have without reservation denounced the wave-theory of sound as fallacious must certainly be entitled to respect and consideration.

If Prof. Helmholtz, Tyndall, Lord Raleigh, Sir William Thomson, in Europe, and Prof. Rood and Mayer, in this country, wish to retain the respect and

confidence of thinking people, they will at once endeavor either to defend the theory of sound, or like men come boldly to the front and acknowledge that it is fallacious. I cannot conceive of an educated scientist, especially one who respects his name and values the opinion others may hold of his ability, integrity, and honor, who could under the present state of facts remain silent; especially if he has been a teacher of the theory which is now not only questioned but denounced as false by prominent scientific professors in our colleges and universities.

Their silence, if such should prove to be the case, could only be attributed to defeat and cowardice. For no matter how well educated they may be, it must be acknowledged that other men are educated also, and that such men command the respect and confidence of thinking people; their opinions are of value, however, only in proportion as their deductions are logical and consistent with facts; so that if they should err in denouncing the current theory of sound,—and if this was demonstrated by an acknowledged supporter of the theory,—such a man who should expose their false deductions would certainly be respected, while a man who is unwilling to defend his assertions is not entitled either to respect or consideration.

AUTHOR.

NOTE.

THE author would be greatly indebted to any reader of this book if he would kindly forward to him any published review or criticism of the same, *whether in favor of or against it.* Such courtesy will be promptly acknowledged.

AUTHOR.

61 BROADWAY, NEW YORK CITY, N. Y., U. S. A.

LECTURE

ON

THE FALLACY OF THE PRESENT THEORY OF SOUND.*

LADIES AND GENTLEMEN:

"The object of science is not to find out what we like or what we dislike; the object of science is truth." Galileo has stated that the truth or falsity of an hypothesis must be judged by the weight of the facts and the force of mathematical deductions, and not by superficial appearances or the plea of authority based about what philosophers may have taught. This line of reasoning clearly applies to the present theory of sound which I have the pleasure of directing your attention to this evening. Simply because the wave-theory of sound was

*Lecture delivered before the New York Academy of Sciences, December 8, 1884. Held at Columbia College.

inaugurated by Pythagoras 2500 years ago, and has been sustained by Newton, Laplace, Helmholtz, Tyndall, Mayer, Rood, Blaserna, and a score of other distinguished scientists, is no reason why such theory should not be judged by "the weight of facts and the force of mathematical deductions."

Too many theories which have had the indorsement of scientists have been exploded to permit of the acceptance of anything in science on trust, or believing anything to be positively correct and true just because it is sanctioned by the indorsement of a long and immortal line of scientific names, especially if the subject has not received the most scrutinizing scientific examination from every possible standpoint.

Theories in science are of great value to deduce explanations for various unsolved problems; and when the correct theory is discovered which will explain and conform with any particular problem under consideration in all its details, as known, then the theory ceases to be a theory and becomes *a law*. The assumption, however, of necessity must always exist, if the law be accepted as correct that we are either acquainted with all the details respecting the problem under consideration, or that if any new facts or details should afterwards turn up they would conform to the law. If they do not so conform, then the so-called law reverts back to the nature of a

theory requiring either modification or complete annihilation. It is to the complete annihilation of the undulatory theory of sound that your attention is directed.

In 1877 Dr. A. Wilford Hall published a work on the Evolution of Sound, in which he carefully considered step by step the present undulatory theory of sound as elucidated by the distinguished authorities already referred to. It is needless for me to say that he has, not only in my opinion, but also in the opinion of numerous scientific men, shown the fallacy of the present theory of sound, otherwise I would not be here this evening to present before a scientific body, in a lecture, the sum and substance of the numerous fallacies pointed out by him and other scientists besides myself who have carefully investigated the subject.

To proceed properly it will be best for us to review with some care the present theory which has been so very generally accepted as correct, and which is at this moment being taught by all the schools and leading universities of the world, with the main object of correctly enlightening students and searchers of truth as to how we hear or appreciate sound. But before I proceed I must respectfully ask your most critical and analytical attention to the present theory of sound, which I shall endeavor to conscientiously lay before you as it is expounded by such men as Tyndall, Helmholtz, Lord

Rayleigh, Mayer, Rood, Sir William Thomson, Blaserna, and other distinguished scientists and recognized authorities on acoustics, with the belief that you will see in the theory, independent of facts I shall give later on, sufficient impossibilities and contradictions to cause you to wonder why such a theory could be entertained at all.

Professor Rood has defined sound as a sensation produced when vibrations of a certain character are excited in the auditory apparatus of the ear, and states that these vibrations are generated by progressive tremors in the atmosphere, called sound-waves.* This definition, while setting forth briefly the present theory of sound, will not be sufficiently elaborate for us to-night, so I will therefore enter into much fuller explanation, based, as I have already stated, upon the propounded facts of the various authorities on this subject.

These authorities state that when a body capable of emitting a (musical) sound—a tuning-fork, for example—vibrates, it moulds the surrounding air into sonorous waves, each of which consists of a condensation and a rarefaction, and in the condensed portion the air is above, and in the rarefied portion is below, the average temperature, and this change of temperature produced by the passage of the sonorous wave itself virtually augments

* Johnson's Cyc.: Acoustics (O. N. Rood).

the elasticity of the air and makes the velocity of sound about one sixth greater than it would be if there were no change of temperature.* But the average temperature of the air is unchanged by the waves of sound, as we cannot have a condensed pulse without having a rarefied one associated with it, and the temperature of the rarefaction is as much lowered as it is raised in the condensation; † so that from a vibrating body, such as the tuning-fork referred to at the end of a second from the time it commenced its vibrations, the foremost wave would have reached a distance of 1095 feet in the air at 0° C. or 1120 feet at 15°C. The prong of the vibrating fork in its swift advancement compresses the air immediately in front of it, causing the particles of air to crowd together, and when it retreats it leaves a partial vacuum behind it as the air-particles separate more widely,‡ the function of the fork being to carve the air into these condensations and rarefactions,§ and they, as they are formed, propagate themselves in succession through the air. If a ball is urged against a number of balls placed in a groove, the motion thus imparted to the first ball is delivered up to the second; the motion of the second is delivered up to the third; the motion of the third is

* Tyndall, Lectures on Sound (1st ed.), pp. 29, 45, 46.

† Tyndall, loc. cit. p. 29.

‡ Tyndall, loc. cit. p. 28.

§ Tyndall, loc. cit. p. 62.

imparted to the fourth: each ball after having given up its motion returning itself to rest, the last ball only of the four flies away. Thus is sound conveyed from particle to particle through the air; the particles filling the cavity of the ear being finally driven against the tympanic membrane, which is stretched across the passage leading to the brain, setting this membrane which closes the drum of the ear into vibration,* so that it bends once in and once out by each pulse.† In this transferrence of the vibrations of the air into the labyrinth, it is to be observed that though the particles of the air themselves have a comparatively large amplitude of vibration, yet their density is so small that they have no great moment of inertia, and consequently when their motion is impeded by the drum-skin of the ear they are not capable of presenting much resistance to such an impediment or of exerting any sensible pressure against it.‡

But still Professor Tyndall says that if we hear one sound louder than another, it is because the ear is hit harder in the one case than in the other by the vibrating air-particle. §

At this point it appears proper to describe briefly the

* Tyndall, loc. cit. pp. 4, 5. † Tyndall, loc. cit. pp. 49–60.
‡ Sensations of Tone (Helmholtz), p. 199.
§ Tyndall, loc. cit. p. 11.

formation of the ear as relates to the *membrana tympani*. This membrane separates the cavity of the tympanum from the bottom of the external meatus. It is a thin semi-transparent membrane, nearly oval in form, somewhat broader above than below, and directed very obliquely downwards and inwards. Its circumference is contained in a groove at the inner end of the meatus, which skirts the circumference of this part except above.

The handle of the malleus descends vertically between the inner and middle layers of this membrane, as far down as its centre, where it is firmly attached, drawing the membrane inwards, so that its outer surface is concave, its inner convex.

"This membrane is composed of three layers, an external (cuticular), a middle (fibrous), and an internal (mucous.)"

"The *fibrous* layer consists of fibrous and elastic tissues. Some of the fibres radiate from near the centre to the circumference; others are arranged in the form of a dense circular ring round the attached margin of the membrane."

The tensor tympani draws the membrana tympani inwards, and thus heightens its tension.

"The laxator tympani draws the malleus outwards, and thus the membrana tympani, especially at its fore-part, is relaxed." The stapedius muscle by inclining

the stapes backwards is supposed to compress the fluid contents of the vestibule.

The tympanum is traversed by a chain of movable bones which connect the membrana tympani with the labyrinth, and serve to convey the vibrations communicated to the membrana tympani across the tympanum to the internal ear. The outer wall is formed by the membrana tympani, a small portion of bone being seen above and below this membrane. It presents three small apertures: the inter-chordæ posterius, the Glaserian fissure, and the inter-chordæ anterius. Through the first the chorda tympani nerve enters the tympanum; the aperture of the inter-chordæ anterius being above the Glaserian fissure, and through it the chorda tympani nerve leaves the tympanum.*

It might here be stated that the membrana tympani is claimed to be capable of responding to an immense variety of waves or impulses. "A catholicity of the kind," says Prof. Rood very truthfully, "has not thus far been observed in experiments on membranes artificially stretched, whose range is found to be far more limited."†

The vibrations produced within the membrana tympani by the waves of sound are transmitted to the mem-

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* See Gray's Anatomy, pp. 697–699.

† Johnson's Cyc.: Acoustics (Rood).

branous covering of the fenestra ovalis by means of the chain of bones within the cavity of the tympanum, and through secondary vibrations produced within this membrane the impulse is transmitted to the fluid of the vestibule. According to some authorities, the jarring of the otoliths against the filaments of the vestibular nerve affords, at this latter point, a perception of the *intensity* of the sound which is being appreciated by the ear.*

The vibrations now travel along the fluids of the scala vestibuli of the cochlea and of the semicircular canals, thus passing in two different directions.

The little sacs † contain, attached to their walls, small crystals of carbonate of lime in contact with the nerves; and their function, as it appears, is to render us sensible of simple short sounds or shocks which probably would not affect the rest of the vibratory apparatus. They act as *drags* on the nerves when the latter vibrate with the water in which they are bathed and thus produce sensation. These sacs contain also, in connection with the nerves, certain microscopic hairs that are quite elastic and brittle, and probably capable of being set into vibration when the particular notes to which they are tuned are presented to them, just exactly as a tuning-fork can be

* Darling and Ranney, Essentials of Anatomy, p. 596.

† Rood, loc. cit.

set in vibration by the waves proceeding from a second fork of the same pitch.

In the semicircular canals, according to some observers, the *direction* from which the sound springs is perceived, while the vibrations carried along the scala vestibuli are transmitted to the filaments of the auditory nerve in the organ of Corti and those connected with the membrana basilaris, thus affording the perception of the note and the quality of the sound perceived. After reaching the apex of the cochlea the vibrations are transmitted from the scala vestibuli downwards along the tympani secundaria, which covers the *fenestra rotunda*, where the vibrations are lost; being no longer transmitted, on account of the absence of any conducting medium. The free entrance of air to the cavity of the tympanum, or the middle ear, affords an equal density of air upon either side of the membrana tympani, and thus insures a vibration of that membrane in absolute unison with the vibrations of the sound which it is called upon to record.*

The function of the organ of Corti in the cochlea is described as follows: When the vibrations reach the end of the auditory nerve, some of the numerous rods or fibres of the Corti arches which bristle around the appendages of this nerve will be strongly excited by such

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* Darling and Ranney, loc. cit. p. 596.

vibrations as are in unison with them, and some will not; so that simple tones of different pitch will excite different fibres, showing that this wonderful organ discovered by Marchese Corti is a musical instrument, with its cords so stretched as to accept the vibrations of different periods and transmit them to the nerve-filaments which traverse the organ.*

If the sound is a compound, or the form of the wave abnormal, this sound is, according to Helmholtz, analyzed into its constituents, since the cords (and rods) can only execute normal vibrations; and we see finally that the clang tint is the sensation produced by the simultaneous action of two or more of these strings upon their appropriate nerve. The cochlea contains about 3000 of these strings, and if with Helmholtz we suppose that 200 of them are useful for rendering us sensible of tones not used in music, there will remain for the musical tones proper 2800 for the seven octaves, or 400 for each octave, $33\frac{1}{3}$ for each half-tone.†

The final vibrations are sent along the auditory nerve to the brain, where they are translated into sound.

The abnormal sounds perceived when the Eustachian tube is obstructed by swelling of the mucous lining during attacks of severe influenza are due, in great

* Tyndall, loc. cit. p. 224.

† Rood, loc. cit.

measure, to the impaired entrance and exit of air. It is customary for gunners when firing large cannon to stand with the mouth open, since by so doing the vibrations of the air produced by the explosion are transmitted through the Eustachian tube as well as through the auditory canal, and by neutralizing each other the drum membrane stands almost motionless, and little, if any, sound is perceived.*

The motion of the sonorous wave, says Tyndall, must not be confounded with the motion of the particles which at any moment form the wave; for during its passage every particle concerned in its transmission makes only a small excursion to and fro, the length of the excursion being the amplitude of the vibration † on which the loudness or intensity of a note depends, as well as it depends on the difference of density between the condensations and rarefactions. So that, if two forks were made to vibrate so that the condensations of the one will coincide with the condensations of the other, and the rarefactions of the one with the rarefactions of the other, thus supporting each other, a sound of greater intensity will be produced than that of either vibrating alone.‡ It is evident, however, that at each point in the mass of air at

* Darling and Ranney, loc. cit. p. 596.

† Tyndall, loc. cit. p. 44.

‡ Tyndall, loc. cit. p. 258.

each instant of time there can be only one single degree of condensation, and that the particles of air can be moving with only one determinate kind of motion, having only one single determinate amount of velocity, and passing in only one single determinate direction.* So that two different degrees of density produced by two different systems of waves cannot coexist in the same place at the same time; although the air is competent to accept and transmit the vibrations of a thousand instruments at the same time,† each particular sound passing through the air as if it alone were present;‡ the pitch of each note depending on the number of aerial waves which strike the air in a second.

Tyndall states that when the tympanic membrane is shaken by the shock of a series of pulses at regular intervals, it cannot come instantly to rest.§ And Helmholtz states that an elastic body set into sympathetic vibration by any tone [whether in unison or not] vibrates sympathetically in the pitch or with the vibrational number of the exciting tone; but as soon as the exciting tone ceases, it goes on sounding in the pitch or vibrational number of its own proper tone.‖ This in the face of the fact stated by him that membranes tuned to be in unison with

* Helmholtz, loc. cit. p. 40.

† Tyndall, loc. cit. p. 287.

‡ Tyndall, loc. cit. p. 281.

§ Tyndall, loc. cit. pp. 49, 69.

‖ Helmholtz, loc. cit. p. 215.

the combinational tones are set in sympathetic vibration immediately upon both generating tones being sounded simultaneously, but remain at rest if only one or other of them is sounded.*

To close the consideration of this side of the undulatory theory, I will add that Tyndall states that when we try to visualize the motions of the air having one thousand separate tones,—to present to the eye of the mind the battling of the pulses, direct and reverberated,—the imagination retires baffled at the attempt.† And he might have added, the shallowness and fallacy of the undulatory theory of sound was made apparent. He, however, does express himself as follows: "Assuredly no question of science ever stood so much in need of revision as this of the transmission of sound through the atmosphere. Slowly, but surely, we mastered the question, and the further we advanced the more plainly it appeared that our reputed knowledge regarding it was erroneous from beginning to end."‡

Now for the other side of the discussion, or to demonstrate what this paper calls for: *The Fallacy of the Present Theory of Sound.*

To do this it will be advisable to divide the considera-

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* Helmholtz, loc. cit. p. 235.

† Tyndall, loc. cit. p. 257.

‡ Tyndall, loc. cit. p. 328, 329.

tion of this subject up into different heads. We will therefore consider (1) the *Agitation of the Air;* (2) *Mobility of the Atmosphere;* (3) *Resonance;* (4) *Heat and Velocity of the supposed Sound-waves;* (5) *Tuning-fork;* (6) *Decrease in Loudness of Sound;* (7) *The Physical Strength of the Locust;* (8) *Barometric Theory of Sir William Thomson;* (9) *Elasticity and Density of the Air;* (10) *Interference and Beats;* and (11) *The Membrana Tympani and the Corti Arches.*

Agitation of the Air.

The wave-theory of sound is inferred from the fact that when a body is excited by a blow or from any cause it is known that the body vibrates more or less, and is capable of inducing the vibration to a greater or less extent of any body with which it is in contact, and in fact will cause the vibration of bodies such as membranes, etc., which are in close proximity to it, provided the two bodies are connected by an elastic medium—such as air, for example. Thus far experiments are able to sustain this; and from these facts it is assumed that instead of the vibration (more strictly speaking, agitation) of the air having a very limited existence, not exceeding thirty feet even from the effect of blowing a steam-whistle, and which agitation does not travel more than five feet in a second, the vibrations or supposed air-waves are sent off and forth, so that a steam-siren can be heard for from twelve to fifteen miles by the final vibration of the membrana tympani by the action of these so-called waves. I fail to find recorded one experiment which shows the existence of vibrations, unless in the immediate vicinity of thier source, except, of course, the

sympathetic vibration of bodies in perfect unison, or experiments conducted in tubes. If it were desirable to show the agitation of the air produced by a vibrating body, the numerous experiments and photographs made of the disturbances produced in the air by a body under such conditions would answer admirably; but that we have a right to assume because there are such vibrations or agitation near the source of the sound, that they extend beyond a certain limit and by condensations and rarefactions of the air produce air-waves which are propagated in some cases for fifty miles, and in other cases over one hundred miles, is too preposterous a proposition to be entertained, as I propose presently to demonstrate.

It is claimed that because a string of an instrument swings greatest when the tone is loudest, hence the loudness of a tone at a distance from the sounding body must necessarily depend on the amplitude of the oscillating air-waves, the supposed law being "that the intensity or loudness of a sound is proportional to the square of the amplitude of the sound-wave."

But unfortunately the oscillating air-waves or agitation of the air, instead of travelling as supposed 1120 feet a second, absolutely do not and cannot move away from the string a total distance of more than a dozen inches, as will be shown.

The bugler may blow directly through his horn without producing tone, and exert all his lung-power and he cannot stir a sensitive gas-jet twelve feet distant; while the air-waves (agitation) he thus produces do not travel more than *four feet a second*, as determined by Dr. Hall. I have failed to cause a candle-flame to flicker when performing the same experiment fifteen inches from the flame. When the bugler adjusts his lips to the mouthpiece in such a manner as to cause the horn and its air-column to generate tone by the proper molecular vibration, he manufactures and sends off the supposed air-waves with less than one fourth the lung-power he employed before, which is supposed to set the entire atmosphere into oscillation throughout 36 cubic miles, causing every particle of air to change its position from a state of rest into a "small excursion to and fro." And still if the instrument is placed within two inches of the candle-flame, I have found that the same will not be affected; and yet not one writer on sound seems to be able to see the difference between the sound of an explosion and its concussive shock, which would knock a man lifeless to the ground if standing near a magazine.

The lowest tone of an organ is stated by Prof. Blaserna to have 16 vibrations to the second, and a consequent wave-length of 70 feet. It thus follows, says

Dr. Hall,* that in the sound of such an organ-pipe the air-particles (as a whole) are obliged to travel 35 feet and back 16 times each second in order to pass from the space occupied by the centre of rarefaction to the centre of condensation and back. They would thus move with a velocity in one direction of 560 feet a second, or at the rate of 381 miles an hour, which would produce a tornado of more than double the velocity necessary to sweep a village into ruins! If there was the least truth in the wave-theory, the sound of a church-organ should get up a cyclone which would blow a cathedral into atoms! For the air, says Prof. Carter, must move with a given velocity and exert a certain pressure, irrespective of the distance through which the air-particles move—a fact no one can dispute.

It would appear at least reasonable to inquire, If a given vibrating body can produce air-waves which can cause the membrana tympani to vibrate and thus produce sound, say, four hundred feet away from it, and that to avoid any possibility of failure of an experiment directed to the object of proving the existence of these so-called sound-waves, such experiments should be conducted at least beyond the known or possible sensitive action of the necessary agitation accompanying any disturbance of a

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*Problem of Human Life: chapter Evolution of Sound, p. 141.

medium in which a body is in the act of vibrating, why are not the experiments conducted, say, one hundred feet away, using funnels to concentrate the air-waves?

The answer is simple: Because sound is not propagated by air-waves, and consequently there are no waves to concentrate. Why is it that no one has thought to attach to the tympanic membrane of the ear a bristle which can rest against a revolving cylinder covered with lamp-black, so that when a sound is made, say, fifty feet off, a register of the supposed vibrations of the membrane of the ear would be made in wave-lines on the cylinder, just as would result from speaking into a tube having a diaphragm at one end, with the bristle attachment placed against a blackened revolving cylinder within the distance of thirty feet from a steam-whistle?

The fact is, there would be no wave-lines produced, because there would be no vibrations of the air at the distance of fifty feet from the source of sound, and the tympanic membrane would not vibrate: and this in the face of the fact that the sound of the human voice can be heard through the air of this locality ten times this distance, and 160 times the distance in the arctic regions over a sheet of ice, as attested to by Captain Parry on his return from his polar expedition.

The experiment given by Prof. Tyndall to show how the waves of sound can cause a candle-flame to flicker

or be extinguished by clapping two books together in front of a long tube directed toward the flame is certainly a most childish experiment; for if he had simply clapped the books together, with the backs of the same to the tube, the flame would not have even flickered and certainly not have been extinguished. It was the puffs of air, in the first instance, which caused the flickering and the extinguishing of the flame, and not the supposed sound-waves. But fearing that some one might think this was the case, he put in the end nearest the book a piece of paper liberating smoke, and because on clapping the book the *first* time no smoke issued from the other end, he held that this was conclusive evidence that the flickering and extinguishing of the flame was due to sound-waves and not to puffs of air. Why did not Prof. Tyndall keep on rapidly clapping the books when they were facing the end of the tube? It was because he knew that the smoke would soon issue and destroy the experiment. There is one thing he took good care to do, and that was to put the smoking paper at the large end of the tube, where it would have a long distance to travel in order to get out, instead of putting it at the end near the candle-flame, when one rapid clapping of the books would have sent the smoke out and destroyed the experiment altogether.

No one has disputed the fact that the human voice, as

well as other sounding instruments, does produce a succession of air-waves capable of passing off for a limited distance around. But these air-waves have been shown to be only an incidental effect of the motion generating the sound, and not by any means the sound itself. For a circumscribed distance around the sounding body the waves, passing off with exactly the force and rapidity of the accompanying sound-discharges, will, of course, by impinging upon a sensitive membrane, throw it into forced vibration, in exact conformity to the original vibration which generated the tone and the accompanying wave-motions which are thus sent off. Such forced tremors occur whether the membrane is in unison with the sounding body or not, but cannot occur outside of the limited distance traversed by such incidental air-waves unless in unison.* As a proof that sound and air-waves are two separate and distinct phenomena, it is evident that the membrane of a telephone could be moved back and forth by any direct mechanical means other than air-waves, such as a delicate system of levers, acting on it with all the variety of rapidity, varying amplitude, and force which govern its motion when certain words are spoken into the mouthpiece; it would produce precisely the same result on the varying intensity of the electric

* Hall, loc. cit. p. 524.

current and strength of magnet, and would consequently reproduce the same variety of movement in the membrane of the telephone at the other end of the line by the receiver, causing the words to be repeated there alone by mechanical means the same as if they had been originally spoken into the mouthpiece by means of the vocal organs. As a proof of this we have only to look at Mr. Edison's phonograph, which actually accomplishes the equivalent of what has just been described, but without levers. Dr. Hall * says: "I have not the least doubt but that the wonderful mechanical genius of an Edison, a Gray, or a Bell can, and possibly will, yet produce a purely mechanical means of operating on such a membrane through some kind of key-board and levers, by which a deaf and dumb person may learn to talk in oral words by the manipulation of keys, the same as he might learn to play a tune on the piano without being able to hear it."

From the marvellously delicate effect developed by Prof. Hughes's instrument by which the step of a fly can be heard through the electric wire miles away, it cannot be possible that the movement of a fly's foot can exert sufficient physical force to alternately compress and expand a solid glass tube or a stick of carbon, and in this

* Loc. cit. p. 525.

manner alternately strengthen and weaken the electric current passing through the wire. Everything tends to favor the opinion being formed by able scientific thinkers that something more than mechanical air-waves is necessary to produce the infinitely delicate effects generated at the transmitting device of a carbon telephone, or microphone as it is sometimes called. Some of the greatest physical investigators do not hesitate to claim that even the more delicate telephonic effect produced through the Bell diaphragm cannot be attributed to its mechanical or bodily vibrations toward and from the pole of the magnetized bar. The eminent Scotch physicist Dr. Ferguson* states as a proof that but a portion of these effects can come from the vibratory motion of the transmitting membrane; that a solid iron plate an inch thick in place of the membrane has produced transmissions of speech, and that even the naked end of a magnetized bar has done the same thing without the intervention of any kind of diaphragm or plate. Dr. Ferguson says: "I would, in the first place, take exception to the vibratory theory of Bell, viz., that it is the vibrations of the disk to and from the pole of the magnet, in excursions proportionate to the intensity, pitch, and quality of the vocal sounds, that electrically affect the

*Sci. Am. Suppl., No. 120.

instrument; and in so doing I only express the dissatisfaction with it of almost every one who deals with the telephone. Consequently mere vibrations of the iron disk are insufficient to account for its action."

He holds that the molecules, which are simply the smallest particles of the iron, are actually displaced and caused to change in their relative position to each other by the action of "external sound," and that this sonorous contact generates currents of electricity. Now, says Dr. Hall, if "external sounds" can actually produce electricity in a steel bar or in the iron disk of a telephone as well as vibratory motion, it is plain that sound must be something more than mechanical air-waves. This view is fully confirmed by Mr. Edison* himself, who says: "I discovered that my principle [the alternate compression and expansion of carbon by sound-waves], unlike all other acoustical devices for the transmission of speech, did not require any vibration of the diaphragm; that, in fact, sound-waves could be transformed into electrical pulses without the movement of any intervening mechanism."

Before closing this section of the subject it may be well to refer to the supposed fact that light, as well as sound, is credited with being produced by wave-motion.

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* Prescott, Telephone, p. 226.

It is well known that sound can be heard* even with one ear closed, and with the open ear turned directly away from the sounding body, and even when shielded from it by a large obstructing surface like that of a building, though, of course, the sound is not so distinctly heard as if the ear opened directly toward the sonorific body, and without any intervening impediment; whereas, light cannot swerve to the right or to the left the smallest fraction of an inch, and cannot be seen at all, even in the slightest degree, unless it enters the eye in a direct line either from the luminous body or from some *reflecting surface.*

If air-waves can lap around the head and enter the ear on exactly the opposite side, then ether waves—if there be such an all-pervading substance as ether, and if there be any truth in the undulatory theory of light—should do the same thing, and thus enable us to see a candle at a distance in a dark night with the back of the head directly toward it. These two results are thus so diametrically opposite that the supposed wave-motion of two perfectly analogous substances—air and ether—cannot explain both. If light be due to wave-motion, then sound cannot be, and vice versa; or perhaps neither one of them is due to wave-motion, which comes nearer the truth.

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* Hall, loc. cit. p. 160.

Mobility of the Atmosphere.

Will any one explain how a pulse can continue on in a direct line in advance of a fork's prong, while at the same time they take into consideration the mobility of the air? It is certainly a well-known fact that the air can get out of the way of a body passing through it and thus pass around and behind it, and neutralize the effect in the disturbed portion, without the motion being transmitted more than a few inches from the centre of disturbance. Because air will move out of a cylinder where it is enclosed when forced to do so by a piston, is no reason why we should assume that a fly, by moving its wings and thus stirring the atmosphere, would actually continue the same displacement "to every other point of the atmosphere." It must be remembered that, *unfortunately* for the wave-theory, the air is not enclosed in a tube. The idea that the mobility of a body is related to or depends on its compressibility could not be entertained, as the mobility of the air which can be compressed to a liquid is about *the same* as quicksilver, one part of which is only compressible in 440,000 parts for each atmosphere of pressure.

How a scientific man such as Tyndall could even "roughly" compare the motion imparted to balls confined in a groove, already referred to, to air-particles possessing lateral mobility,* and which are free to slip around and not practically be pushed at all, is certainly, to say the least, incomprehensible. The credit of referring to the mobility of the atmosphere for the first time in any work on sound is due to Dr. Hall; and it alone, if no other argument could be advanced, is sufficient to destroy the wave-motion or spring-power of the air unless confined.

* Hall, loc. cit. p. 261.

Resonance.

Tyndall states that "so thin a body" as a harp-string vibrating to and fro, while capable of moulding the air into condensations and rarefactions travelling with a velocity of about 1100 feet a second,* cannot throw the air into sonorous vibrations, and that this effect is produced by the large surface with which the string is associated. This idea is certainly untenable. No one disputes that the large surface or board of the harp necessarily receives a tremor from the vibrating string bearing against it, but such tremor can only be regarded as incidental, or *as the effect of the motion which produces the tone* and not such motion itself. If viewed otherwise, the sounding-board would be the controlling mechanism in the production of tone; and consequently, instead of playing a secondary part to the string, which has but a hundredth part of the vibratory effect on the air, the board should take possession of the sound and change its pitch to its own vibratory rate, whereas the string, not a thousandth part so large in area, retains its

* Tyndall, loc. cit. p. 88.

perfect pitch, mastering and annihilating that of its powerful coadjutor: and this must be true of all stringed instruments besides the harp, such as the lute, piano, and violin.* If the wave-theory were correct, that resonance is really caused by the vibratory motion of the board, then evidently each string as soon as sounded should lose its own identity and be forced to conform to the normal pitch of the sounding-board; but this is not true, as the tone of a string never changes its pitch in being transferred to and augmented by the sounding-board: and we all can see how important it is that it should not, for in the case of the piano, which has 85 separate strings, all having their own rate of vibration or pitch which we wish to preserve, the string vibration would be lost in the one vibrational pitch of the sounding-board, which we know does really augment the sound by resonance. Hence resonance must receive some other explanation than that given by the wave-theory.†

Tyndall sets forth in his work under the head of Resonance an experiment to show the length of the column of air in a jar which resounds to a tuning-fork. Using one which vibrated 256 times a second, and of necessity having a length of 4 feet 4 inches to its sonorous wave, he detached it from its case and struck it

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* Hall, loc. cit. pp. 83, 84.

† Hall, loc. cit. p. 84.

against a pad, the sound produced being hardly audible; he then held it over a glass jar and poured in water very gently until the column of air reached the proper height, when the sound burst forth with extraordinary power. The re-enforcement of the sound is known as resonance. By measuring the length of this column of air he found it to be 13 inches. The length of the wave emitted by the fork is 52 inches. Hence, says Tyndall, the length of the column of air which resounds to the fork is equal to one fourth of the length of the sound-waves produced by the fork.

If this statement were true it would argue in favor of the wave-theory; but unfortunately Dr. Hall found that a straight jar gave the greatest resonant depth not at 13 inches but at 11¾ inches, thus making the wave-length 47 inches instead of 52, as it should be according to the wave-theory. This would make the velocity of sound 1002 feet a second at 60° F., instead of 1120 feet a second. Perhaps Laplace might orginate a new formula of heat and cold to account for this discrepancy of 118 feet a second.

To obtain a resonant depth of 13 inches, it would be necessary to expand the mouth of the jar and carry the expansion a sufficient distance down; but such a jar is not an ordinary straight jar, and must be expanded to great nicety to produce the required result. A straight jar will

invariably give a resonant depth of $11\frac{3}{4}$ inches. A jar with a given bell-shaped mouth gave a depth of $12\frac{1}{4}$ inches, while one with a contracted mouth gave a depth of $11\frac{1}{4}$. The honest straight jar gave, as stated, always $11\frac{3}{4}$ inches: and this disputes the wave-theory of sound by just 118 feet a second. These experiments can be easily verified and the fallacy of the wave-theory demonstrated.

Heat and Velocity of Sound-Waves.

Newton, independently of the necessities of the wave-theory, calculated the exact relative density and elasticity of the air, which when applied to the admitted requirements of the theory made the velocity of sound in air at the freezing temperature 916 feet in a second, whereas the well-known observed velocity was 1090 feet, thus showing an undeniable discrepancy of 174 feet a second between the observed and the required velocity, or a deficit of about *one sixth* against the wave-hypothesis.

When this fact was made known, the undulatory theory of sound should have been abandoned as a practical absurdity contradicting in its fundamental principles the observed facts of nature. Laplace, the great French mathematician, came to the front and proposed using the heat generated in the condensations of the supposed sound-waves.

This was based on the fact that sound travels faster in heated air than in cold; and as heat also adds to the elasticity of the compressed portion of the wave, it was assumed that the excursion of the air-molecules into the heated or condensed portion and out again would be executed more rapidly than if no heat or augmentation of elasticity were

generated ; and hence it was concluded that the velocity of a given sound would be sufficiently increased by this change of temperature to make up the required 174 feet a second, or add the one sixth discrepancy pointed out by Newton.* So that "an augmentation of velocity of about two feet for every single degree centigrade" † is obtained, from which we deduce that for 174 feet the condensed half of a sound-wave must be raised 87 C. (156°.6 F.). Surely the actual distance the air-particles would have to travel to and fro cannot be "infinitesimally small" if by such motion any appreciable generation of heat and cold is to be expected. The real question which is of interest here is the amount of pressure to the square inch, in avoirdupois, which would be required to generate the 87° C. or 156°.6 F. necessary for the additional velocity of 174 feet a second.

To determine this point,‡ an experiment was conducted in a glass tube having a thermometer in it, as also a tight-fitting piston, when Dr. Hall found, making liberal allowances, that with a pressure of 15 pounds to the square inch the actual heat generated was five degrees. Applying this deduction, if we find a cubic inch of air requires 15 pounds pressure (reducing it to one half its bulk) to raise its temperature 5 degrees, how much pressure will it

* Tyndall, loc. cit. pp. 29, 45, 46.

† Tyndall, loc. cit. p. 25.

‡ Hall, loc. cit. p. 144.

require to raise the temperature of the same cubic inch 87 degrees? Rejecting fractions, we have 255 pounds as the result.

Therefore any sound in passing through the air must produce an atmospheric pressure in the condensed portion of its waves of 255 pounds to each cubic inch to raise its temperature 87° C., which is necessary to add 174 feet a second to the velocity of sound. If this does not look absurd, it would be only necessary to calculate the absolute pressure which a mosquito must produce throughout a room of given dimensions in order to generate sufficient heat to thus add one sixth to the velocity of its sound.

One more illustration may be interesting; and we will select iron this time as the medium, through which sound travels 16,822 feet a second. The same condensations and rarefactions take place in iron as in air, and heat must be generated by each condensation; but unfortunately when iron is once heated to any degree whatever it cannot instantly become cool, so that heat generated by one condensation of the iron could not have time to subside in any calculable degree before its re-enforcement by another, that by another, and so on, at the rate of 440 a second, if the pitch should be that of A, or the same as that of the second string of the violin. "It is thus," says Dr. Hall,*

* Hall, loc. cit. p. 235.

"perfectly manifest, according to the wave-theory, that a locust by singing for one minute, sitting on a mass of iron, ought to raise its temperature to incandescence; for however little heat a single condensation would produce, this rapid accumulation, without time for subsidence, would necessarily accomplish this miraculous result."

But as a fact there is no heat generated by the passage of a sound; there can be no condensations and consequently no waves of sound.

According to General Duane and other scientific men of our Signal Service as well as in the service of other nations, fog-horns and steam-sirens are often heard many times further against a violent gale than with it.

Tyndall recognizes this fact, which he verified by his experiments off South Foreland.* Yet Prof. Rood states it has been found "experimentally that sound moves quicker with the wind and slower against it, the final velocity being in the one case equal to the sum, in the other equal to the difference, of the velocity of the wind and that of the sound-wave itself."† It is no wonder that Tyndall said, "Plainly, therefore, something else than the wind must be influential in determining the range of sound." Surely, if sound is attributable to air-waves, such waves even from the most powerful fog-horn in the service could not travel against a gale even a dozen feet.

* Tyndall, loc. cit. p. 296. † Johnson Cyc.

The Tuning-Fork.

With the above facts before us, let us look into the distance travelled in one second by the prongs of a tuning-folk or string of an instrument.

Prof. Carter has conducted an experiment on the slow movement of a tuning-fork's prongs, first shown by Hall, which is certainly worthy of careful consideration. The following is the result of his experiment as stated by him: *

"I used a large Koenig fork of 256 vibrations. Striking it heavily and holding it upright in my fingers, I found that its sound was clearly audible, either held to the ear or through a long rubber tube, at the end of *four minutes.* By means of a finely graduated scale I easily measured the amplitude of the fork's swing. I found it to be at first $\frac{4}{60}$ ($\frac{1}{15}$) of an inch. At the end of fifteen seconds it had reduced to $\frac{1}{60}$ of an inch amplitude. At the end of fifteen seconds more its motion was barely visible against the sky. Now, I can see a line of $\frac{1}{240}$ of an inch in breadth, which proves that the amplitude had again diminished to one fourth. In the third fifteen seconds

* *Microcosm*, Dec. 1883, p. 154.

the motion had become totally invisible even through a good magnifier. Safe to assume another fourth, or a reduction of amplitude to $\frac{1}{960}$ of an inch for each swing.

"Now there are sixteen times fifteen seconds in four minutes; hence I have the $\frac{1}{15}$ of an inch swing reduced by four as a divisor sixteen times, or in round numbers to $\frac{1}{64000000000}$ of an inch at each swing. As the prong swings through this amplitude, counting both directions, 512 times in a second, we have the entire distance the prong travels, while sounding audibly, but the $\frac{1}{123000000}$ of an inch in a second. There are in round numbers 31,500,000 seconds in one year. Hence the prong moves at the rate of only about one inch in four years. Allowing one half for the swifter travel of the prong at the centre as compared with its average travel throughout a swing [which is too much], and we have the astounding fact that the fork continues to produce audible sound while its prongs, at their swiftest motion, are not travelling at a velocity of more than one inch in two years. . . . Let physicists dispose of these figures, if they can, or forever after hold their peace."

Helmholtz * says: "The pendulum swings from right to left with a uniform motion. . . . Near to either end of its path it moves slowly, and in the middle fast. Among

* Helmholtz, loc. cit. p. 28.

sonorous bodies which move in the same way, only very much faster, we may mention tuning-forks." Dr. Hall says: "We now earnestly ask every candid student of science to examine this unavoidable teaching of the wave-theory in the light of the absolute facts here developed, that the prongs instead of 'swiftly advancing' (as Tyndall says) sound audibly when moving more than 25,000 times slower than the hour-hand of a family clock, and more than 300,000,000 times slower than any clock-pendulum ever constructed, instead of 'very much faster' as Helmholtz teaches. Surely with such an overwhelming demonstration as this against the truth of a theory of science, such theory ought not longer to bear sway over the minds of intelligent teachers in our colleges and universities, nor be longer taught as true science to the perversion of the minds of young students." *

The question has been asked me, which one of two conical pendulums is moving more rapidly—one that makes one swing around a circle to travel, say, ten inches in one second, or a pendulum which makes, say, 100 swings in a second but whose aggregate circular travel is only ten inches in one second. The impression was that the pendulum making 100 swings in a second unquestionably moved the faster.

* *Microcosm*, Dec. 1883, p. 155.

The absurdity of this line of thinking I made apparent by a simple illustration: Given three men who can walk one mile in ten minutes; one proposes to walk once around a circle exactly one mile in length, another four times around a circle one quarter of a mile in length, and another ten times around a circle—one tenth of a mile in length—all three accomplishing the aggregate mile in ten minutes. Surely the three men walk at the same speed or velocity, and their collision against an object would be exactly the same, and their effect upon the air in the view of compressing or condensing it would be exactly the same.

Again, given a conical pendulum which by one swing travels around a circle whose circumference is one inch and does it in one second, and a conical pendulum making 100 circular swings in one second but whose aggregate distance of travel is only half an inch in that second. It must be plain that the conical pendulum making 100 swings in one second is only travelling with one half the speed or velocity of the other, and can compress or condense an object only one half the extent that the first could.

Let us take another illustration, for this argument is fatal to the wave-theory, and it alone shows the impossibility of the so-called undulatory motion of the air in the production of sound. Now let us compare the

conical pendulum to the reciprocating pendulum. Assume a conical pendulum swinging around a circle whose diameter is the length of swing of any particular reciprocating pendulum. Again, assume the reciprocating pendulum swinging forward and back to the extent of the diameter of such circle. Now it is a fact, attested to by Prof. Meyer, and one which can be easily verified, that at the centre of the width of swing or amplitude of the reciprocating pendulum, which of necessity is the centre of the circle, the velocity of the pendulum at this point is as swift or great as the velocity of the conical pendulum at any point in its path or swing.

The reciprocating pendulum, however, in the same time does not travel as far or as great a distance as the conical pendulum. This is owing to the stops, starts, and slowing of motion from the centre of its swing towards its ends. Its greatest velocity as stated at the centre, however, is the same, no greater and no less. The difference in the distance travelled forward and back is *substantially* equal to the difference between twice the diameter of a circle and its circumference.

Again, the swiftest motion of a reciprocating pendulum in going and returning, including stops, starts, and slowing of motion, is *substantially* as much faster than its average motion as the circle is greater than twice its diameter.

In other words, while the reciprocating pendulum is making one excursion in one direction, the conical pendulum is making one half the circumference, or *about* one and a half, while the other is making one. The swiftest velocity, therefore, of the reciprocating pendulum in one excursion is *about* one half times faster than its average velocity.

It is a well-known fact that a pendulum of a given length takes just as long to vibrate or swing one inch forward and back as it would to vibrate or swing 100 inches forward and back. This is due simply to the fact that to make it move through 100 inches it requires more force and consequently it goes faster than it would if only moving through one inch. This must not be mistaken to mean that the pendulum moves or swings as rapidly through the one inch as it does through the 100 inches; the fact is, while it makes the swing in the same time it goes just 100 times slower.

What we are interested in, therefore, is not the time it takes a pendulum or a prong of a tuning-fork to swing through a given distance, but what velocity of motion the pendulum or prong has at any period of its motion.

The velocity of a prong of a tuning-fork and the distance it travels in progressive swings are always on the decrease. The number of vibrations do not change; hence the pitch is maintained. Its intensity is what varies, and

does so as the amplitude of the prong's vibration becomes less, and it continues to decrease until the prong comes to rest.

Lord Raleigh,* speaking of sonorous waves, says it is necessary to have "a knowledge of the energy which must be expended in a given time in order to generate them." Nothing could be more important to us right here. It is the velocity of a body that hits or is capable of compressing or condensing, and not the stops and starts which one man said to me was the cause of the prong of a tuning-fork condensing air in front of it. Just imagine a prong of a tuning-fork while standing still or at a stop condensing anything. This is too absurd to dwell on.

Now, as I have clearly shown, as the amplitude becomes less and less while the tuning-fork sounds, the velocity of the prong also becomes less and less, and the blow it can give, or its supposed ability to condense the air, becomes less and less as it keeps on sounding. Let us therefore apply these facts, which I challenge any one to contradict, to the results obtained by Prof. Carter. It will be remembered that with a fork, after being struck, making 256 vibrations, he was able to hear the same distinctly for four minutes, during which time the

* *Nature* (1877), vol. xvi. p. 114.

amplitude of the prong's swing had diminished to $\frac{1}{64000000000}$ of an inch; and as the prong swings through this amplitude, counting both directions, 512 times in a second, the entire distance the prong travelled, while still sounding audibly, was only $\frac{1}{123000000}$ of an inch in one second, or, at its swiftest motion, only one inch in two years—more accurately, one and one-half inches in four years.

Now, I would submit the following proposition: Assume a prong moving at a velocity no faster than the hour-hand of a clock. Does any one pretend to say that such a prong could compress the particles of the air into condensations and by its retreat leave a "partial vacuum" behind it, or a rarefaction, and thus send off sonorous air-waves?

It is certain Tyndall * does not believe any such nonsense, for he says: "When a common pendulum oscillates it tends to form a condensation in front and a rarefaction behind. But it is only a *tendency;* the motion is so slow, and the air is so elastic, that it moves away in front before it is sensibly condensed, and fills the space behind before it can become sensibly dilated. Hence waves or pulses are not generated by the pendulum." Now, this being admitted by one of the highest authori-

* Tyndall (3d ed.), loc. cit. p. 38.

ties on the current system of acoustics, will some one please enlighten me how a prong when only travelling one and one-half inches in four years, or travelling 25,000 times slower than the hour-hand of a clock already referred to, or 300,000,000 times slower than any clock-pendulum ever constructed, can "mould" or "carve the air" into condensations and rarefactions and thus produce sound-waves? Do not forget the fact that a prong of a tuning-fork advancing and retreating only one inch in one second is not only going with a velocity 123,000,000 times greater than the prong of the tuning-fork in Carter's experiment, which made 256 double vibrations, but it can exercise 123,000,000 times the force towards condensing the air.

No, gentlemen! Your wave-theory has been shown by the above to be incorrect, absurd, and fallacious, and you might as well reject the same and look around for some other theory that will not make its supporters the laughing-stock of intelligent men.

Before leaving this part of the subject it will be well to refer briefly to the

Frequent Change of Direction of the Prongs of a Tuning-Fork.

It has been suggested that when Tyndall and other authorities on acoustics speak of the prong of a tuning-fork "swiftly advancing," they really refer to "rapid change of direction," so that by the rapid or, more properly, "frequent" change of direction of the prong of a tuning-fork the air is moulded or carved into condensations and rarefactions. For when a strip of iron say four feet in length is put in a vise and pulled aside and then let go, while it will oscillate transversely, it will not produce sound, for its oscillation or change of direction can be counted; if, however, the strip be removed and again adjusted so that only two feet of it are now free to move, it will oscillate four times as frequently; if one foot free, it will oscillate sixteen times as frequently as at first; if six inches free, sixty-four times as frequently, and so on. The oscillations now become so rapid, the number of them in a second (i.e., their frequency) become so great, that they can no longer be counted directly; we now hear sound—the shorter the

vibrating part the more rapid becomes the vibration, consequently the shriller the sound.*

Again, it has been suggested that when a body oscillates in a large circular swing covering, say, ten feet, it ought to stir the air only gradually; but when the body having the same length of cord oscillates 100 times around a circle having a circumference of $\frac{10}{100}$ of a foot, it travels the aggregate distance of ten feet in the same time. The same air is acted upon oftener in this case than in the former, and it has not time to regain its normal position before it is acted on again and again, and for this reason the air ought to be churned up more in this case than in the former. Applying this same argument to the prong of a tuning-fork in its excursion to and fro, it follows that, as no sound is heard until the vibration or change in direction of the prong is very frequent, the cause of the formation of sonorous waves is due to this churning motion or agitation. Let us examine this supposed plausible solution of the problem, and if its absurdity can be shown, the hope of forming sonorous waves by a vibrating body will be completely blasted, as it has been shown that the slow motion of a tuning-fork's prong cannot produce them, and that the stops and starts cannot produce them, and therefore they cannot be produced at all, which is unquestionably the case.

* See Daniels, Prin. Phys. p. 366.

It must not be forgotten that the particles constituting a sonorous wave must make a small excursion to and fro to the extent of the amplitude on which the intensity depends. To induce a particle to make such excursion it is evident that the particle must be hit, and this particle must hit its neighboring particle, and the new particle thus hit must also perform its excursion, and so on with the rest of the particles constituting the wave. Now to hit the first particle it must be agreed that the prong of a fork must advance and must have some velocity, and certainly more than a clock-pendulum, which on account of its slow motion, according to Tyndall, only tends to form a sonorous wave, and that it is only a *tendency*. Daniels* also states that "air will not oscillate in waves such as can be propagated to a distance, unless there be some well-marked compression or rarefaction produced at the centre of disturbance." . . . "A vibrating body before it can act as a sounding body must produce alternate compressions and rarefactions in the air, and these must be well marked. If, however, the vibrating body be so small that at each oscillation the surrounding air has time to *flow round* it, there is at every oscillation a local rearrangement—a local flow and reflow of the air; but the air at a distance is almost wholly unaf-

* Loc. cit., p. 367.

fected by this." "As a general rule it is advisable," says Daniels, "when sound is to be heard at a distance, to make the sources of sound of the largest size convenient. Smallness of size may, however, be compensated by quickness of vibration." "Thus the chirp of certain insects is produced by such extremely rapid movements—as many as 12,000 to-and-fro vibrations per second—that the air is alternately compressed and rarefied on each side of the wings or in the neighborhood of the stridulating organs without having time to flow around them." It is evident from this that even Prof. Alfred Daniel seems to indicate that "frequent change of direction" is equivalent to velocity of motion, and that "frequent change of direction" can itself actually produce condensation and rarefaction. The fallacy of such ideas I now propose undertaking to show.

It has been shown that the prong in Carter's experiment while sounding audibly did not travel but $\frac{1}{64000000000}$ of an inch, and its greatest aggregate travel was only at the rate of $1\frac{1}{2}$ inches in four years.

Try and form some conception of the meaning of such figures, if possible. If you cannot, bear in mind this one fact: that even one billion (say nothing of 64) exceeds all the seconds of time in thirty-two years.

To illustrate by values within the range of our intellectual grasp. Suppose a body, a lead-pencil for ex-

ample, to be moved slowly through the air at the rate of *one inch in an hour*, and that at the end of an inch it stops, and then immediately goes another inch in an hour, either in the same direction or in the opposite direction: would such motion, if repeated a thousand or a million times, condense the air and send off a pulse? Any one would answer, No. Would the same rate of motion divided into halves—that is, half an inch in half an hour, and then stop, and repeat another half-inch in half an hour, and so on—come any nearer condensing the air? Common-sense answers, No! Then let the pencil move a *sixtieth of an inch* at the same velocity, that is, *in one minute*, and stop, and then move another sixtieth in another minute and stop, and so on a thousand times, the same distance at the same velocity each time: could such reduction of distance and division of time change the result or the effect on the air if the same velocity were maintained? An ignoramus would intelligently answer, No! He could also understand that no possible division of the original inch and the original hour into segments could condense the air, and that millions of stops and starts of the pencil during the hour, each motion having no greater velocity, would produce no more effect on the air in the way of condensing it than would the first single motion of an inch in an hour. Thus when the prong in Prof. Carter's experiment

moved the $\frac{1}{64000000000}$ of an inch at one swing, or at the velocity of $1\frac{1}{2}$ inches in four years, it could no more condense the air or drive off a pulse than would the same prong travelling $1\frac{1}{2}$ inches in four years at one continuous motion. Surely no educated man could come to any other conclusion.

It must therefore be conceded that nothing but the swift velocity of a body through the air can produce an atmospheric pulse. No matter whether the movement of a body be long or short—whether it consist of one motion or a succession of motions in one direction, or a succession of motions in opposite directions—slow motion in a fluid or mobile medium can only displace the particles. Should the prong of the tuning-fork in Carter's experiment stop and start in one and the same direction at no greater velocity, it could no more condense the air by the second movement than by the first; nor could its stops and its starts in the opposite direction at no greater velocity produce any different result than by its first movement. If two such movements at such velocity could no more compress the air than could one, then three, five, ten, ten-thousand, or ten-million such separate motions, each at no greater velocity, would be incapable of producing a condensation or sending off a pulse. This is certainly an axiomatic and self-evident truth. Once more I ask you to bear in mind that it is

agreed that velocity of motion is all there is to consider, since the stops manifestly can do nothing, being motionless. After a body has *stopped* moving *there cannot be the slightest difference which way it goes* as to the compressing effect on the air, whether in the direction it was pursuing before it stopped, in the opposite direction, or in some other direction. Its effect on the air will be precisely the same, according alone to its velocity. If its velocity is too slow to condense the air at any one motion, then two, ten, or a million similar motions can add nothing to velocity or condensing power. *Hence the numerous changes of direction constitute a factor entirely outside of this problem of condensing the air*, since each separate movement is to be considered independently, or by itself, the same as if no other movement had been or was to be made; and being almost infinitely too slow to send off a condensation or pulse, it demonstrates the wave-theory of sound to be false.

President Barnard states:* "If the foreign body [a prong of a tuning-fork or other body] exerts but one impulse and then remains at rest, only a single tremor will pass through the medium and there will be no vibration." This is not even theoretically true; for a single forward motion and stop should produce a wave,

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* Johnson's Cyc.: Vibration.

but the time required should be longer; the air-particle would move forward and be compressed, and would form a condensation. When the particle recoils after imparting its compression, a rarefaction of necessity must be produced, but not so quick as when the moving prong moves back or recedes as fast as it advances.

Let us now inquire into the diameter of the particles of air and see if we cannot get a better idea of what the prong of a fork is supposed to do. Loschmidt says: "A cube whose sides is the $\frac{1}{4000}$ of a millimetre (.039368 inch) (= 0.00000984 inch) may be taken as the *minimum visible* for observers of the present day. Such a cube would contain from 60 to 100 million molecules of oxygen and nitrogen." And when we take the results obtained by Clerk Maxwell, Dupré, Lorenszy, and Sir William Thomson, the diameter of the molecule is found to be between $\frac{1}{250000000}$ to $\frac{1}{5000000000}$ of an inch. To form some conception (says Thomson) of the degree of coarse-grainedness indicated by these conclusions, imagine a rain-drop, or a globe of glass as large as a pea, to be magnified to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be coarser-grained than a heap of small shot, but probably less coarse-grained than a heap of cricket-balls. So much for their size; and as regards their weight, Maxwell says it would require two hundred

million million million molecules of hydrogen to weigh one milligram (1 milligram = 0.01543 grain); and further, that the distance between them is 200 times greater than their diameter. Now, Prof. Rood has stated that the amplitude of sound-waves varies roughly from $\frac{1}{50}$ to $\frac{1}{1000000}$ of an inch; while Lord Raleigh has determined that the note F^{iv}, which has 2730 vibrations (and a consequentwave-length of less than five inches), has an amplitude of somewhat less than one ten-millionth of a centimetre, which is equal to one two-hundred-and-fifty-four thousandth ($\frac{1}{254000}$) of an inch. Now Tyndall distinctly states * that the time required by a particle to execute a complete oscillation is that required by a sonorous wave to move through a distance equal to its own length. Supposing the length of the wave to be 8 feet, and the velocity of sound in air of our present temperature to be 1120 feet a second, the wave in question will pass over its own length of air in $\frac{1}{140}$ of a second; and this is the time required by every air-particle that it passes to complete an oscillation, for at the end of a second from the time the prong of a tuning-fork commenced its vibration the foremost wave would have reached a distance of 1120 feet. In large organs the lowest note is C″, with 16½ vibrations, having a wave-length

* Sound (3d ed.), p. 97.

of over 60 feet; the highest musical note of an orchestra being probably the D^{v} of the piccolo-flute, with 4752 vibrations, having a wave-length of 2.8 inches. The range, however, of the ear is given by Helmholtz and Depretz between 16 vibrations, with a consequent wave-length of 70 feet, and 38,000 vibrations, with a consequent wave-length of 0.352 inch, or about one third of an inch. Taking the lowest note, then, the length of time required for a particle of air to make a complete oscillation is $\frac{1}{16}$ of a second, and the highest $\frac{1}{38000}$ of a second.

In Carter's experiment the length of time a particle of air is supposed to oscillate is $\frac{1}{256}$ of a second, as there are supposed to be just 256 sonorous waves of four feet four inches in the first 1120 feet, and so on. Now let us apply these facts. Assume the prong of the fork in Carter's experiment to advance $\frac{1}{64000000000}$ of an inch, it is exceedingly questionable if it would hit a molecule at all. As I have stated, the space between the molecule is 200 times its diameter; and second, the distance travelled by the prong is over 12 times less than the diameter of the smallest of molecules, or 256 times less than the diameter of the largest of molecules. It is evident that the prong would have to do some fine engineering to strike even one of the largest of molecules, assuming that an oxygen or nitrogen molecule is as large as $\frac{1}{250000000}$ of an inch. Now as the prongs of this fork make only

256 vibrations in one second, it is quite certain that only one molecule could be hit in one second, if any are hit at all. Suppose the fork did hit a molecule: what good would that do? for according to the current theory the molecules of the air at 60° F. are in rapid vibration; and if they were hit, the slow motion of the prong surely could not affect them sensibly. If a man attempts to hit a ball so as to send it off in the air to a distance, he brings his bat around with great velocity; if he did otherwise the ball would simply drop down in front of him. So that if the molecule is not hit with considerably swifter velocity than a pendulum could hit it, no sensible effect would be produced. In Carter's experiment it was shown that the prong advanced at each vibration $\frac{1}{64000000000}$ of an inch. Now, as Prof. Rood has stated that the amplitude of an air-particle varies roughly between $\frac{1}{50}$ and $\frac{1}{1000000}$ of an inch, according to the intensity, let us take the smallest distance, say the $\frac{1}{1000000}$ of an inch. In so doing we find that the prong in Carter's experiment travels a distance 64,000 times less than the amplitude of the particle taken; and as every vibration of the prong is supposed to form a new wave, and the length of time in Carter's experiment the particle has to complete its excursion to and fro is $\frac{1}{256}$ part of a second, as the prong vibrates 256 times in the second, the air-particle must of necessity move to and fro in each case at least 64,000 times faster than the

prong advances, or than its velocity. Just imagine a bullet travelling "its distance" (for an air-particle has "its distance" to travel, which is its amplitude) at 64,000 times greater the velocity than the gases which gave it its motion. The above argument has nothing to do with the transmission of the pulse, but simply refers to the motion of a particle of air and of the distance it must travel in its excursion. It must certainly be admitted that reason disappears when such things are believed in. It therefore must be clear to every one by this time that a vibrating body cannot produce condensations and rarefactions and thus send off sonorous waves. Hence the wave-theory of sound is false and must of necessity be abandoned, except perhaps to illustrate how plausible a theory can be and still at the same time not be vested with one element of truth.

The present hypothesis compels scientists to assume that there are two entirely distinct principles of wave-motion in the atmosphere: one suited to their sound-theory, which will travel 1120 feet a second; and another class adapted to common-sense, which will not move more than four or five feet a second—both manufactured in substantially the same manner. For example, "they all know* and would readily admit, if a string or a piece of wire be

* Hall, loc. cit. p. 118.

moved back and forth in my hand through the air with the most perfect pendulous regularity, and caused to travel at an aggregate velocity even ten times greater than it is possible for it to obtain when sounding, that the air-waves will not travel over four or five feet a second, if that fast, and will not be able to make headway through the dense air a dozen feet till they will entirely die out. But the moment the same string moves through the air with its two ends supported in such a manner as to generate tone, though with an aggregate velocity not one tenth as great, then, presto! it sends off air-waves, according to these learned physicists, which travel 1120 feet a second, or more than 200 times as fast!"

If the string oscillates less than sixteen times a second it makes no sound, and consequently the air-waves are slow waves; but if it should oscillate forty or fifty times, then the waves start off with a velocity of 1120 feet a second. Can any well-balanced intellect see either consistency, sense, or science in this arbitrary and absurd distinction?

As sound-waves are claimed to be "of a precisely similar nature" and "essentially identical" and move "exactly in the same way as water-waves," as is claimed by Tyndall, Helmholtz, and other authorities, we must devote a few minutes to this comparison. By an original investigation, Dr. Hall, after experimenting with water-

waves of all sizes, has deduced the following law hitherto unobserved by any writer on sound: "*that wave-velocity is always exactly in proportion to wave-length, or distance from crest to crest.*" This law, then, inevitably breaks down the wave-theory of sound, since it is a well-known fact, and universally admitted by physicists, that there is no difference in sound-velocity under the same conditions of temperature, etc., between the highest notes, such as D, of the piccolo-flute, with a theoretical wave-length of less than three inches, and the low E, for example, of the double bass, with a theoretical wave-length in air of twenty-eight feet. This must of necessity be the case, for a band can be heard a quarter of a mile away the same as a few feet away, and each sound, whether high or low, reaches the ear in perfect time: the rhythmical relation to each other would be destroyed by any difference in velocity. The velocity, however, of water-waves diminishes with the distance, and their wave-lengths contract or shorten in the exact ratio as their amplitude becomes less. These being facts, then, a wave of sound should travel slower and slower the farther it gets away from the generating instruments, while it should also become higher and higher in pitch by the contraction of its wave-lengths, as this is exactly the manner in which water-waves are propagated.

In water-waves, any wave which happens to be a small

fraction larger than the one preceding it must necessarily gain slowly on the one in advance, till at last overtaking it the two blend into a single wave of about double the normal size of waves constituting that system; the same thing then continues, until at last immense *king-waves* are produced. Now if sound consisted of wave-motion at all, or if air-waves were possible as the cause of sound-phenomena, we should certainly hear in every sustained musical tone an occasional outburst, or sonorous explosion, whenever one of the atmospheric *king-waves* should happen to accumulate and dash against the tympanic membrane. As no such sonorous effects are ever observed, it becomes clearly manifest that sound does not travel by means of air-waves at all, or by any principle analogous to undulatory motion.

Decrease in the Loudness of Sound.

Tyndall,* speaking about the propagation of sound from an exploding balloon, says: "Take the case of a shell of air of a certain thickness with a radius of one foot, reckoned from the centre of explosion. A shell of air of the same thickness but of two feet radius will contain four times the quantity of matter; if its radius be three feet, it will contain nine times the quantity of matter, and so on. Therefore the quantity of matter set in motion augments as the square of the distance from the centre of the explosion. The intensity or loudness of sound diminishes in the same proportion."

Tyndall has not taken into consideration that the entire range of many sounds is less than a foot. Take the midge, for example. According to the law, then, if the distance from the midge be two feet, the loudness of the sound will be one fourth; if the distance be three feet, the loudness will be one ninth; if the distance be four feet, the loudness will be one sixteenth, and so on. Yet the sound entirely ceases within a single foot. To employ "feet," then, in computing the rates of decrease in the

* Tyndall (1st ed.), loc. cit. p. 10.

loudness of the sound of a gnat would be to measure about as much out of proportion one way as it would be enormously too small when applied to the sound of the siren.

Dr. Hall says: "We may state it as a truism which no one will question, that the measure to be employed in computing such proportional decrease in the intensity of particular sounds, if we estimate by the square of the distance at all, must always and of necessity vary exactly in proportion to the intensity of the different sounds at the start; or in other words, as the range of the different sounds varies."

The supposed law that the intensity of sound diminishes as the square of the distance is recognized and claimed by all wave-theorists, and in support of this an experiment is freely cited in the text-books. "Four bells at forty feet will exactly equal in intensity of sound one bell at twenty feet." "There is not," says Prof. Carter, "an acoustician on earth who has ever publicly questioned that statement and experiment until Hall's work appeared—and here I make the sweeping statement that not one of the 'trained experimenters' ever once tried to perform that experiment, but simply took it in theory alone. *This is a fatal mistake.* I myself first tried the experiment with a very complete apparatus, and was astonished to find that instead of four equalling one at double distance, four equalled one at *thirty times* the distance."

The Physical Strength of the Locust.

There is a well-known insect, one of the locustidæ (a saltatorial family of the order orthoptera), whose stridulations can be heard a distance of more than a mile, as attested to by Darwin and others. This insect weighs less than a quarter of a pennyweight, and can, by simply rasping its legs across the nervures of its wings (for this is the way its tone is produced), according to the wave-theory, create a physical agitation and displacement of the air which converts four cubic miles of atmosphere into waves consisting of condensations and rarefactions, the compressed portion of which contain a sufficient augmentation of heat above the normal heat of the atmosphere to add one sixth to the elasticity of the air and the velocity of sound. Dr. Hall has made some calculations which cannot fail to be interesting, showing what the consequence of this would be if true.

In the four square miles in which this insect can be heard there are in round numbers 16,000,000,000 square-inch columns of air, each exerting a pressure on the earth and in all directions of about fifteen pounds—in the aggregate 120,000,000 tons. Now, since sound can *only travel* by means of "air-waves," and as air-waves can be con-

stituted only of condensations and rarefactions, and as a condensation can *only take place* by the particles of air, as Prof. Tyndall says, "*crowding closely together*," or a rarefaction occur except by the particles of air separating "*more widely apart*," and as every particle of air constituting a sound-wave, according to the same high authority, must necessarily make "a small excursion to and fro" every time a wave passes, it inevitably follows, if this theory be true, that this insect, by simply moving his legs, displaces all the particles of air constituting these 16,000,000,000 inch columns for a mile high, and restores them to their place again 440 times each second (its tone being very nearly A, or that of the second string of the violin), and continues this process of thus churning the atmosphere into condensations and rarefactions a full minute at a time.*

When we apply the power exercised by the locust (that could not move an ounce of weight) to the membrana tympani, we obtain some additional information which is still more enlightening and interesting.. It is agreed by all scientists that if an ear were present in any portion of the four cubic miles of air, the sound of the locust could be heard and the membrana tympani would bend once in and once out by each vibration, or

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*Hall, loc. cit. p. 130.

440 times in one second of time. It is evident that there is in this membrane some weight or inertia to overcome; this weight Dr. Hall has determined to be at least half a grain for each membrane. Now, by a simple calculation which any schoolboy can verify, it is found that there is room enough in this area, in round numbers, for 65,000,000,000,000,000 of these tympanic membranes, as the membrane is not over a quarter of an inch square: which would give us a ponderable mass of 4,000,000,000,000 lbs., or two thousand million tons of tympanic membrane, which this trifling insect, according to the wave-theory of sound, is capable of throwing into rapid vibratory motion by the mechanical operation of moving its legs, moving the same once in and once out 440 times a second. As forty million people (nearly the whole population of the United States) could conveniently stand within four square miles permeated by the sound of this insect, their 5000 lbs. of tympanic membrane would bend once in and once out 440 times in one second. And as the whole population of the earth is estimated at 1350 million, and as this number of individuals could stand conveniently in eleven and one half square miles, their 231,222 lbs. of tympanic membrane would, according to the wave-theory, bend once in and once out, about 1000 times a second, by the sound from a steam-siren, or 440 times a second by the stridulation of thirty-four locusts.

Another illustration will, I think, be all that is necessary to establish the value of the wave-theory of sound. We will select iron this time as our medium instead of air, as we did in the case of the illustrations on heat.

Imagine a locust stridulating in the centre of a mass of iron one mile in all directions. It is admitted that he could be heard, and about sixteen times quicker than in the air, by placing the tympanic membrane in actual contact with the iron at its surface. Now, since a mass of iron is all thrown into wave-motion, and since, according to Prof. Rood, the amplitude of waves of sound will vary roughly from $\frac{1}{5}$ to $\frac{1}{1000000}$ of an inch, the iron must be moved at least to this extent; which could be easily verified, as Prof. Rood has been able to measure $\frac{1}{2000000}$ part of an inch. The mass of iron thus displaced would weigh not less than 729,749,050,612 tons, and would be so moved by the strength of the locust, as it is permeated by its sound in all directions, as in the case of air, and every molecule must perform the wave-amplitude of the theory, or an excursion to and fro.

The wave-length would naturally be over sixteen times as great as in air. It would follow from this, if the note were the low E of the double bass, which has forty vibrations to the second, that the length of the iron wave would be of necessity 476 feet from crest to crest.

It may be well to mention here that Prof. Mayer

states * that a given sound in passing through the atmosphere and producing its condensations actually increases the "density" of the "compressed half" of the wave $\frac{1}{679}$ over the normal density of the air. Applying this, to the determination of the physical strength of the locust we have the modest amount of five thousand million tons; while if the calculation is based on the estimated heat which this pressure must necessarily generate to meet the requirements of Laplace these figures are thrown into the shade, making the physical energy of the locust equal to 132,566,207,938,560,000 lbs., or in round numbers 66,000,000,000,000 tons. †

On the theory of Sir William Thomson, ‡ the locust could only produce sound according to the law which changes barometric pressure,—that is, by the changes in the weight of the atmosphere,—as will be shown farther on. Therefore, as the locust can be heard over four square miles of the earth's surface, or over an area of 15,844,448,400 square inches, its mechanical strength, by moving its vibratory apparatus, can add 60,000,000 lbs., in round numbers, to the weight of the atmosphere. For if the barometer rises only one tenth of an inch, it shows that the weight of the atmosphere has actually increased

* See Appleton's Encyc.; article Sound, by Alfred Mayer.

† Hall, loc. cit. p. 145.

‡ Sci. Amer. Suppl.

34 grains to each square inch on the surface of the earth at that locality in order to produce such change.

Surely this fact is sufficient to show the absurdity of the current doctrine of acoustics.

Prof. R. Kelso Carter* has made some interesting calculations (which do not seem out of place here) in relation to the bell employed by Messrs. Colladon & Sturm under water to determine the velocity of sound. Two questions, says Carter, must be asked:

1. What caused the actual motion of the water?

2. How much was actually (not theoretically) moved?

In this problem the actual amplitude of the supposed vibration of the water-particles is not of the slightest consequence. The fact is the particles moved, and moved at a specified rate [within their amplitude].

Carter deduced the following undeniable facts:

1. This remarkable bell (the sound of which was heard nine miles off) actually set in vibration particles of water in twenty cubic miles [estimated nine miles long and broad, with a depth of one fourth of a mile]. 2. This amount of water weighs 920 trillion tons. 3. The dead-weight resistance offered by this water to every impulse amounts to two trillion tons. (This supposes the impulse to be given at the smallest cross-section.) 4. This dead-

* *Microcosm*, vol. iii. p. 262.

weight was positively overcome 400 times in a second as long as the bell was heard.

In this calculation, to meet the objection which might be urged that the sounding body only moves the first layer of the water, and the motion is communicated and handed over to the next with some loss, and so on, it makes no difference how thick or how thin a "first layer" be taken. Carter has taken no thickness at all. It is indisputable that the amount of square surface will offer the resistance he has given. The only questions are:

1. How great is the cross-section of the water moved?
2. How much resistance to such a rate of motion does water offer to the square foot?

It is a principle laid down by Archimedes that any force whatever which proceeds or radiates from a centre diminishes as the square of the distance along any given line. Now suppose the bell in the above case to vibrate one inch. Then the initial impulse given to the water will be one inch in amplitude. While the molecules of water next the bell will move one inch, no one can invent a reason for the next molecule to move any farther. Applying the above-mentioned principles, Carter finds as the force goes on, at nine miles it would be reduced to $\frac{1}{324000000000}$ of an inch, which is less than $\frac{1}{600}$ of the diameter of an ultimate molecule, so called, of hydrogen. But as this molecule is supposed to have a vibratory path

200 times its diameter, then the $\frac{1}{6000}$ is reduced to $\frac{1}{250000}$. As the vibratory motion of the hydrogen molecule in its path has never been heard, it is plain that an impulse to affect the auditory nerve must exceed it, as it is about thirty times less.

This argument of Prof. Carter certainly settles the locust question, and sustains the deductions made to show what a locust would be called on to do if the wave-theory of sound were correct.

The Barometric Theory of Sir William Thomson.

As this is the very latest interpretation of the present theory of sound, it seems appropriate to devote a few minutes to its consideration.

According to Sir William,* as stated briefly before, sound is "exceedingly sudden changes of pressure acting on the tympanum of the ear, through such a short time and with such moderate force as not to hurt it, but to give rise to a very distinct sensation which is communicated through a train of bones to the auditory nerve." To explain: It is a "sudden change of pressure," and it differs from a gradual change of pressure" as seen on the barometer only in being more rapid—so rapid that we perceive it as sound.

If by any means a fall of the barometer could happen "amounting to a tenth of an inch and taking place in a thousandth of a second," says Sir William, "it would affect us quite like sound: a sudden rise of the barometer would produce a sound analogous to what 'happens' when I clap my hands."

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* Sci. Am. Suppl.

By means of a regular barometric tube, with its sensitive column of mercury completely exposed to the air of a room by removing the cork from the enlarged portion of the chamber at its base, so that the slightest change of atmospheric pressure might be instantly observed in the rise or fall of the top of the column, the instrument being properly secured at a convenient height for close observation with a powerful magnifier, Dr. Hall, with the assistance of careful scientific witnesses, deduced the following:* that "No vibratory or wave motion of the air caused by a moving body, let the disturbances or pulses be slow or rapid, produces the slightest effect upon the barometer even in a closed room, and directly at the exposed mercury, as demonstrated by actual experiment. Therefore sound is not produced in our sensations by air-waves or atmospheric pulses sent off from a vibrating body, and consequently the wave-theory breaks down in the hands of its greatest modern champion."

* *Microcosm,* Nov. 1884, p. 125.

Elasticity and Density.

Tyndall states a supposed law as follows:

"The velocity of sound in air depends on the elasticity of the air in relation to its density. The greater the elasticity the swifter is the propagation; the greater the density the slower is the propagation."

What, then, is elasticity? It is a property of a body analogous to that of ductility, malleability, porosity, fusibility, etc., and in no manner or degree does it or can it exert mechanical force or aid in overcoming the inertia of a body: "its whole office* being to permit a certain kind of motion or quality of effect through the application of adequate mechanical force." So that the elasticity of the air could not add one grain of force to the original mechanical energy of the locust, in the illustration already given, by which it could displace the air by overcoming its inertia.

It must of necessity be conceded, if the above law were correct,—if a body could be found having great density and no elasticity,—sound should not travel through such

* *Microcosm*, vol. iii. p. 219.

a substance at all. Such a body we have practically in lead, which is one of the densest metals and is almost entirely devoid of elasticity. Miller says: * "By elasticity we understand the resistance that a body offers to compression or to extension, and the property which it possesses of regaining its former volume when the pressure or tension is withdrawn." When heat is applied to lead, its particles are so soft that they slide over each other, according to the current theory, in the act of expansion, and do not return to their original position. A leaden pipe of a few feet long, if used for conveying steam, becomes permanently lengthened by some inches in a short time. † This would not be the case if lead had more elasticity. How, then, is it possible for lead to transmit sound-undulations 4030 feet per second, as Tyndall states, when the velocity of sound through water is practically the same, or 4714 feet? The density of lead is just about eleven times that of water, and Tyndall says, "Other things remaining the same, an augmentation of density always produces a diminution of velocity." Hence the velocity of sound in lead should certainly be very much slower than in water, if it should have any velocity at all, according to the law. Tyndall further states that "elasticity is measured by compressibility," as

* Chem. Phys., part i. p. 48.

† Miller, loc. cit. p. 289.

is stated in as many words by Miller's definition of elasticity just given. Which is the more compressible, lead or water, when the coefficient of compression of sea-water is known to be only 0.0000436, and lead, as is known, can be compressed in the cold, being similar to putty?

Professor Tyndall gives the velocity of sound along the fibre of pine-wood as 10,900 feet, and across the rings 4611. Now, says Prof. Carter,* in the name of common reason, is not pine-wood infinitely more compressible than water? Is it not therefore much less elastic? And is it not also only a little lighter or less dense? Well, then, put these facts together, and as the densities are very nearly alike, surely the velocities ought to be the same.

If it is maintained that the elasticity of the air constantly adds force to the impulse imparted to the same by the locust, so that the locust does not have to exert much force to set the atmosphere into vibrations, either, says Dr. B. S. Taylor, "the doctrine of the unalterable quantity of energy must be given up or else the undulatory theory of sound" must be abandoned. This doctrine, however, is perfectly safe, for elasticity, as already stated, is a mere property of a body and cannot add one grain of force to the supposed work of the locust,

* *Microcosm*, Nov. 1884, p. 104.

or the work the locust would of necessity have to perform if the wave theory of sound were correct; but as it is not correct, we can protect the locust as well as the doctrine of the constant equivalence of energy.

As I have just had occasion to refer to the "impulse," it is best for me to say a few words about it, for the majority of people think that all a vibrating body has to do is to start or form a so-called sonorous wave and the "impulse" will safely carry it at the rate of 1120 feet through the air. "An impulse is the effect of a blow," and "the measure of an impulse in producing a change of velocity of a body is the increased (or decreased) momentum produced in the body." "The work done by an impulse (being) measured in the same way as for finite forces." All the effects, therefore, says DeVolson Wood, of an impulse are measured in the same way as the total effects produced by a finite force.

The idea of an impulse contributing one grain of force over and above, or even exactly as much as, the force that excited it, is absurd. What interests us here is the velocity of an impulse. Newton thought an impulse was transmitted through a molecule of a body instantaneously. This is incorrect, even according to the present theory of the constitution of bodies. Matter is supposed to be composed of molecules acted upon by attractive and repulsive forces. Molecules are therefore

not in contact according to this view. If the distance between the molecules be increased within the limits of the action of the forces, both forces are diminished; and if the distance is lessened, both are increased, but not in the same proportion. Applying these facts, then, a molecule of air after receiving a blow is first compressed (which requires time) before it can move towards its neighbor in front of it; and when so moving it excites and increases the forces; the neighboring molecule is then compressed, each molecule performing its own motion forward and back, and so on until the original force is overcome. From this it is evident that in the transferrence of a pulse a given amount of time is required and a given amount of the original force is utilized to do work.

Now, it is a fact that the impulse in an elastic medium travels with considerable rapidity; but such rapidity can be easily ascertained by experiment, and has been done.

If the propagation of sound was attributed to some other cause than the vibrations of condensations and rarefactions of the air, no physicist would assume (for it is a mere assumption) that an impulse can travel at the rate of 1120 feet a second through air. It is only on account of the first assumption that an "impulse" has been credited with such a wonderful performance. Experiments made within the domain of practical limits (leaving the consideration of the so-called light-pulses and sound-pulses out) do not justify any such conclusion.

Interference and Beats.

It is claimed that the so-called sound-waves under certain conditions produce interference, and this is claimed to be especially conclusive evidence in favor of atmospheric wave-motion. It is certainly essential to the theory that such interference should exist, as sound-waves, according to Helmholtz and other scientists, are "essentially identical" with and "precisely similar" to water-waves; as when two equal systems of water-waves travel together in such manner that the *crests* of one system coincide with or fall into the *furrows* of the other system they will mutually destroy or neutralize each other producing a level, or nearly so. So it is claimed that if the condensation of one wave of sound should fall into the rarefaction of another wave of sound of the same intensity, according to Tyndall the result would be "absolute silence;" while if the condensation of the one coincided with the condensation of the other, and the rarefactions the same way, then by this "coincidence" a sound will be produced which will be four times the intensity of either. This supposed law of interference of sound-waves has no foundation in science

or fact, as I will now undertake to show; and if I succeed in doing so, the so-called wave-theory will not have a foundation to stand on.

The double siren, which must be familiar to you all, is an apparatus by which it is claimed that the so-called interference can be demonstrated. To use Tyndall's own words:* "Where the circle is perforated by 12 orifices, the rotation through $\frac{1}{24}$ of its circumference causes the apertures of the upper wind-chest to be closed at the precise moment when those of the lower siren are opened, and vice versa. It is plain, therefore, that the intervals between the puffs of the lower siren, which correspond to the rarefactions of its sonorous waves, are here filled by the puffs or condensations of the upper siren. In fact, the condensations of the one coincide with the rarefactions of the other, and the absolute extinction of the sound of both sirens is the consequence." Tyndall further states that this statement may seem to exceed "the truth; for when the handle is placed in the position which corresponds to absolute extinction, you still have a distinct sound." He claims that the "acrial disturbance breaks up into secondary waves which associate themselves with the primary waves of the instrument." The turning of the upper siren through $\frac{1}{24}$ of its circum-

* Tyndall, loc. cit. p. 291.

ference utterly extinguishes the *fundamental tone.* But we do not extinguish *its octave.* To discuss the merits of this seems too absurd, for "In the name of science and reason—in the name of acoustics and common-sense—what should have been expected but this very result?" * By operating the two sirens together, so that the 12 puffs of the upper siren alternated with the 12 puffs of the lower siren, surely 24 puffs were obtained the same as if he had used one siren with 24 holes instead of 12. The result of necessity must be the raising of the fundamental note to its octave. Tyndall should have known this instead of resorting to talk about "secondary waves associating themselves with primary waves," etc., when he distinctly teaches that no octave, from whatever instrument, can be produced without doubling the number of vibrations which caused its fundamental tone.

Prof. Helmholtz was the first to make the mistake. He says:† "The puffs of air in one box occur exactly in the middle between those of the other, and the two prime tones mutually destroy each other. . . . Hence in the new position the tone is weaker because it is deprived of several of its partials [over-tones]; but it does not entirely cease; it rather jumps up an octave." We have heard the reason why "the tone is *weaker*" in the

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* Hall, loc. cit. p. 288. † Helmholtz, loc. cit. p. 240.

new position according to Helmholtz and Tyndall, and now we will hear the true explanation given by Dr. Hall: That the weaker tone is due simply to the fact that it was constituted of a single series of 24 successive puffs or vibrations to a revolution, while the prime tone was composed of two series of 12 double or unison puffs which necessarily re-enforced each other, and by which means their intensity was increased fourfold.*

Prof. Tyndall showed how the number of vibrations of a particular fork could be ascertained by the siren. He says the siren makes 334 puffs of air in a second, and he further distinctly says that one puff is one wave of sound. With the siren, then, a single puff of air escaping through a hole in a disk constitutes a vibration, or a pulse, or a sound-wave. But with the fork the case is very different. Here we see that a single puff or motion does not constitute a vibration, but only half a vibration.† In the matter of the siren, says Prof. Carter, ‡ 384 puffs of air issue from a tube in a second. Each puff is a motion forward from the mouth of the tube; there is no backward motion in any sense whatever.

Now we would like Prof. Tyndall to explain how a puff of air, that has only one motion "to" and not "fro," can, by his own definition, be called a wave of sound or

* Hall, loc. cit. p. 295. † Tyndall, loc. cit. p. 69

‡ *Microcosm*, Dec. 1883, p. 145.

a vibration. He insists on it that a vibration mu consist of two motions equally distinct, one forward and the other backward, or else it is only a "semi-vibration."

The puff is not suddenly checked and reversed as in the case of a fork's prong, but is left to expend itself against the yielding atmosphere. The stream is suddenly cut off, but no one will be so foolish as to claim that the stoppage of the supply of wind is a reversed motion. If it is, then the stoppage of wind constitutes the vibrating body, for the Professor assures us that the vibrating body must make the "excursion to and fro."

If any one declares that the starting and stopping of the air constitutes the necessary double motion, they can be referred to the tuning-fork, which manifestly stops and starts at each end of its swing, or four times in a complete excursion to and fro.

The facts are that the puff of air makes only a "semi-vibration," or a single motion forward, while the fork-prong makes a double motion "to and fro;" and notwithstanding this difference, the siren and the fork produce precisely the same note when listened to in the ordinary way.

The siren produces sound by using the air itself as the agent, and conveys the sound through itself to the ear. In this case one motion or semi-vibration produces the

effect on the ear of certain notes in the scale. The fork produces sound in itself and does not convey this sound to the ear at all, but is forced to hand it over to another medium, air, to convey it to the ear. In this case a double vibration is necessary to produce the same note. Prof. Carter asks the following question: If sound is produced by a vibrating piece of steel, and were my ear buried in the steel itself, would I hear anything? By the aid of Tyndall's admission that a single air-puff actually produces the same effect on the ear as a double fork-vibration, Carter has been able to discover that "When the ear is in the vibrating body a single or semi vibration produces the same effect as that produced by a double or complete vibration when the ear is not in the sounding body."

If two unison * forks be sounded over the resonant jars of proper depth placed one half a wave apart, their tone can be heard exactly the same in line as at right angles, or when a whole wave-length apart; while according to the testimony of Prof. Helmholtz the very highest authority on the subject, *such sounds are destitute of accompanying over-tones.*

The necessity for referring to "clang-tints" or "over-tones" of the "highly composite" siren resulting from

* Hall, loc. cit. p. 287.

its "secondary waves which associate themselves with the primary waves" is at once dispelled. The real value of the siren, then, is to show that the pitch of every fundamental sound, from whatever instrument, corresponds precisely to the number of vibrations in a second which generates the tone.*

Let us consider for one moment a harp-string. Here we have condensations sent off on one side by the very identical motion which generates and sends off the rarefactions on the other side of the string, and at exactly the same instant of time. So that, according to the theory of interference by half wave-lengths, the rarefactions on one side of the string would react and reflect upward a given distance, just in time to coalesce with the condensation from the other side, since they occur synchronously and both travel with the same velocity; and hence the two systems of waves from the two sides of the string must necessarily produce complete interference and cause "absolute silence" in a vertical direction, if there is a shadow of truth in the wave-theory.

It seems needless to say you know that no such interference takes place. It may be advisable to refer briefly to the apparatus of M. Koenig, the principle of which was first proposed by Sir John Herschel, the ob-

* Hall, loc. cit. p. 302.

ject being to divide a stream of sound into two branches of different lengths and afterward cause the branches to reunite and to interfere with each other. Koenig's apparatus, which must be familiar to you, carries out this idea. Tyndall states that when the parts are properly adjusted the sound of a fork is extinguished. This statement is false, for no such thing as silence occurs, since the sound of the fork is not diminished in intensity more than one quarter, as any sound-expert would readily admit; and by a modification of Koenig's instrument the effect has been proved to be due to the result of resonance, and due to either the re-enforcement or opposition of the two vibrating air-columns of the two tubes;* and it has been further shown that any resonant effect produced by dividing the sound into two streams can be equally obtained by a single stream in connection with a closed resonant tube of certain depth. As the Koenig apparatus has always been considered as conclusive evidence in favor of this law of interference between sound-waves, scientists cannot help but be indebted to Dr. Hall for exposing its fallacy and thus annihilate completely the supposed law of interference, the very foundation of the present wave-theory of sound.

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* Hall, loc. cit. p. 311.

Beats.

If we employ two organ-pipes which give slightly different tones, assuming both their waves start fairly together, the supposed condensations and rarefactions being in harmony, this state of things cannot long remain so, owing to the inequality of their length. Hence in this experiment we must expect to have alternations of sound and silence, the tone rising and swelling to a maximum, then dying away again to repeat itself, etc. These alternations are called beats, and furnish even to the unmusical ear a very accurate means of judging of the identity of musical tones.

According to Helmholtz, discord is due to the presence of the beats; and Rood * states "that when from any cause these beats follow each other at the rate of about 33 in a second the discord is at its maximum, becoming more tolerable with twice this number, and finally disappears altogether as their number is increased to about 120 in a second. On the other hand, if the beats follow quite slowly—for example, at the rate of three

* Johnson's Cyc.

to five in a second—the effect is not unpleasant, and can even be employed in music, suggesting as it does the idea of trilling. It is evident from this that the sinking and swelling of the sounds of two beating instruments result "alone," according to the wave-theory, from the alternate coincidence or interference of the air-waves themselves sent off from such sounding bodies. This I deny, as I have clearly shown that no such interference between two supposed systems of air-waves can take place, since not the slightest weakening of two unison tones occurs when two vibrating bodies are sounded half a wave-length apart. Dr. Hall * maintains that the operation which alternately augments and diminishes the intensity of tone, as the oscillations of two forks cross each other's path in changing from synchronous to alternate vibration, has nothing to do with air-waves or any motion of the air-particles whatever, but takes place in the instruments themselves or in their potential and practical sympathetic attraction for each other, without regard to the coincidence or interference of such useless nonentities as these so-called atmospheric condensations and rarefactions.

Suppose, for example, two forks mounted upon their resonant cases and tuned sufficiently out of unison to pro-

* Hall, loc. cit. p. 305.

duce, say, one beat to the second. If sounded in close proximity to each other or in a position of strong sympathetic attraction, a listener stationed a hundred feet away from them will distinctly hear their beats—will in fact hear them as far away as the sound of the forks is audible. But let the two forks while sounding be quietly separated only a few feet toward the right and left of the listener, and though he will continue to hear their united sounds in full force, yet the beats will entirely cease, showing that they result from the sympathetic influence of the two forks upon each other owing to their affinity, and not to the alternate interference and coincidence of the two systems of supposed air-waves a hundred feet away, or at the ear of the distant observer, as the wave-theory teaches.

The Membrana Tympani and the Corti Arches.

The description of the membrana tympani has been given already, so only a few additional facts will be of interest.

The upper border * of this membrane is 7 mm. nearer to the entrance of the external auditory canal than the lower. The posterior border is about 5 mm. nearer this entrance or meatus than the anterior. It makes an angle of 55° with the axis of the auditory canal. Its general shape is elliptical or funnel-shaped. The horizontal diameter is 8 to 8.5 mm., and vertical 8.5 to 9 mm. Its thickness is not quite 0.1 mm.; in other words, about the same as very fine letter paper. Mr. Shrapnell considers the function of the membrana flacceda is to protect the more tense fibres, deadening the effect of the sudden and loud sound, or of coughing and sneezing, when by yielding it saves the tense fibres from being ruptured. In the hare and sheep, whose sense of hearing is very acute, this structure is remarkably developed.† The existence of a

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* St. John Roosa, Diseases of the Ear (1884).

† London Med. Gaz., vol. x. p. 120.

minute opening in the membrane at its upper margin called the Rivinian foramen has been proved, as air may be occasionally heard to whistle through it, although it cannot be seen by the unaided eye.*

Double hearing sometimes occurs, and Mr. Homes explains it by a defective action of the radiated muscle, which was not exerted with the same quickness and force in one ear as in the other, so that the sound was half a note too low, as well as later in being impressed upon the organ. Nearly all such cases observed are amongst musicians.†

The membrana tympani when viewed through the auditory canal shows a triangular spot of light. The apex of this spot is at its greatest concavity and is called the umbo, and is due to this and the inclination of the membrane. It is reflected light.‡

According to Dr. St. John Roosa, § "the presence of the membrana tympani in whole or in part is not essential to fair hearing power." This was first clearly proved by Sir Astley Cooper. ‖

Roosa, however, says "that it is very important to good hearing in some cases, [as] is shown by the numerous in-

* Kessel-Stricker, Handbook of Histology, p. 953.

† Phil. Trans. of Royal Soc. London, 1880, pt. i.

‡ Politzer, Membrana Tympani, p. 26. § Loc. cit. p. 252.

‖ Phil. Trans. London, 1800, p. 155.

stances in which an artificial membrana tympani raises the hearing power from a very low degree to a higher one." This fact can be readily explained, and will be below.

Sir Astley Cooper * was the first to puncture the membrana tympani to cure deafness, and he cites four cases which by such treatment were made to hear distinctly.

Hunold punctured every membrana tympani to which he could get access, and he reported the brilliant result of curing or improving seventy cases out of a hundred. While this has been questioned, Michaelis succeeded in improving the hearing of 3300 of the cases treated by him. Karl Himly wrote a commentary upon the operation, to show that it was only in exceptional cases that puncturing the membrane was of any value. The truth of the matter, as it appears to me, is that if it were not for the middle or inner ear being liable to get inflamed or diseased, the puncturing of the membrane would be performed very much oftener than it is.

Sufficient cases on record prove, however, what we are more interested in—that, for the perception of sound, the membrana tympani can be completely removed and the subject will hear as well, if not better, without it as with it. Therefore the beautiful idea of the necessity of the

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* Phil. Trans. Royal Soc. London, 1801.

tympanic membrane vibrating once in and once out for each vibration and being hit harder by the air-particles to produce intense notes than if notes less intense are sounded, or the idea that such membrane is necessary at all except to oblige a supposed wave-theory, I think from the above is clearly established, as the medical student referred to by Sir Astley Cooper in his work lost his tympanic membrane but could hear very well. The real value of the tympanic membrane is then simply to keep the cold and dust out of the middle ear, which otherwise might become inflamed or diseased.

Just imagine how accommodating, says Prof. Cox,* the tympanic membrane is supposed to be by the high authorities. If the notes C^2 (256 vibrations), E^2 (320), G^2 (384), and C^3 (512) of the piano be struck simultaneously, according to the wave-theory this membrane is supposed to vibrate within the same second in and out 256, 320, 384, and 512 times. And each sound-wave is supposed to go through the air as if it alone were present. Does this not look absurd, and is it not a valuable discovery which proves that sound-perception does not depend on the vibration of the membrana tympani at all? When the middle ear is diseased, the hearing can be benefited by a hearing-trumpet or audiphone.

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* *Microcosm*, Sept. 1884, p. 60.

Sir William Thomson in his lecture coolly instances a case of a deaf man hearing music by holding a stick between his teeth pressed against the piano while he was playing it, not seeming to recognize the fact * that this conduction of sound to the auditory nerve by means of the solid bones of the head was a flat overturn of his pretentious barometric philosophy.

Prof. Helmholtz states that the drum-skin, as well as every other membrane, must have some definite pitch as the "the vibrational number of its own proper tone;" hence has claimed to have proved that this membrane is toned to a relatively deep note as a result of its funnel-like shape. He says that the tone cannot be exactly determined, but it is certainly not higher than 700 vibrations. So that if the highest note but one in a seven-octave piano-forte (G), with 3400 vibrations in a second, should be sounded, the tympanic membrane at once is coerced into an abnormal rate of nearly 3000 oscillations out of tune, assuming the vibrational number of the membrane to be 440, or the note A, and this too by "sympathetic vibration;" and then, contrary to all known mechanical or acoustical laws, it drops that motion and takes up a new rate of 440 vibrations a second, without any known or exciting cause whatever to superinduce it, since

* Hall, loc. cit. p. 204.

we are told by Helmholtz that "as soon as the exciting tone ceases it goes on sounding in the pitch or the vibrational number of its own tone." "Hence A must be sounding," says Hall, "in my ear as a perpetual *monotone*, while an orchestra is playing, filling up every interval which occurs in any piece of music I hear."

As the vibrational number of any stretched membrane depends on its *size*, *weight*, and *tension*, and as it is evident that no two drum-skins can combine these elements to exactly the same degree in different individuals, it follows that with one person A would be the predominant or loud note, with another B or B♭, with another C or C♯, with another D, and so on through the chromatic scale, or possibly through several octaves. Fortunately, however, such trash as this need not confuse us, as I have already shown that the tympanic membrane can be punctured or removed and the subject will hear as well, and in many cases better, without it as with it.

A word before closing, in relation to the rods of Corti, which have already been referred to in the first part of this paper. Helmholtz insists that they "must be differently tuned and their tones form a regular progressive series of degrees through the whole extent of the musical scale." The "differently tuned" strings of a piano-forte in order to produce its seven octaves are not only compelled to vary in length from $5\frac{1}{2}$ feet to $1\frac{5}{8}$

inches, the difference being as 1 to 40; the size and weight must also differ, the weight of the highest and lowest strings of the piano-forte in order to "form a regular progressive series of degrees through the whole extent of the musical scale" being 1 to about 1600.

Hensen, however, shows that the difference between the longest and shortest of these Corti rods is only about one half, or as 1 to 2, while no perceptible difference in size is recorded which is necessary in the strings of a piano-forte. C. Hasse has shown that these microscopic processes, so essential to the wave-theory of sound, have no existence *at all in the ears of birds!* Yet the mocking-bird can distinguish, analyze, and imitate the finest shade of pitch equal to a prima donna! Away, then, to the winds goes the Corti lute of 3000 strings, as Prof. Tyndall calls it.

As regards the unisonant vibration of the antennæ or so-called "auditory hairs" of the mysis or opossum-shrimp, as also the vibration of the so-called variously tuned fibrils of the antennæ of the male mosquito as spoken of by Mayer,* these motions must be regarded as simply reactive instead of unisonant, being first heard by the animal through the proper auditory organs without any motion whatever of such parts, and then reflected

* Am. Jour. Sci., Aug. 1874.

back upon the antennæ or fibrillæ through the nervous system of the creature, thus causing the tremor which is noticed by experimenters as the supposed direct result of unisonant action. The *filing of a saw* or some peculiar *scraping movement of a slate-pencil*, for instance, will often react through the nervous system unpleasantly upon the teeth, and with some temperaments, so to set them on edge as to be almost unendurable. No one, of course, would suppose that such impressions on the teeth could occur from the direct or objective action of sound-pulses, since a deaf person would perceive no such effect.

Let scientists, says Dr. Hall, test the question of unisonant vibration of antennæ on a dead opossum-shrimp or a mosquito, if they wish to show the absurdity of their deductions.

Conclusion.

In conclusion of this lecture, I would state that numerous other arguments could be added to show conclusively the fallacy of the wave-theory of the sound, but time will not permit; and I question whether any more arguments can be necessary, for Prof. Huxley * has said: "Every hypothesis is bound to explain, or at any rate not to be inconsistent with, the whole of the facts it professes to account for; and if there is a single one of these facts which can be shown to be inconsistent with (I do not merely mean inexplicable by, but contrary to) the hypothesis, such hypothesis falls to the ground—it is worth nothing. One fact with which it is positively inconsistent is worth as much and is as powerful in negativing the hypothesis as five hundred."

My object this evening, as I have stated before, was to show the fallacy of the wave-theory of sound first demonstrated by Dr. Hall, and to point out just such facts as Huxley speaks of, and to show that it is a fallacy of science handed down from age to age like the Ptolemaic

* Origin of Species, p. 140.

system of astronomy till a Copernicus arose, and his *aide-de-camp* Galileo, to show the world a more excellent system.

Now, gentlemen, while I submit the arguments and facts presented in this paper to your careful consideration, with the hope that you will weigh the facts and mathematical deductions with the greatest of care and with the one view before you of searching for truth and accepting the same when found, I am willing to risk the fallacy of the wave-theory upon the correctness of one single objection, and that is the slow instead of swift movement of the tuning-fork when sounding audibly and its inability to produce sonorous sound-waves at all as required by the current theory of acoustics.

If any scientist can fairly and logically meet and answer this argument, I will gracefully acquiesce. Otherwise the wave-theory should be abandoned at once as a mistake, for one single fact which is positively opposed to an hypothesis, according to Huxley, overturns it as completely as would five hundred such opposing facts.

I know some scientist will say, We adopt the current theory provisionally for want of a better one. This I propose to give at some future time. All that I ask now is to have the fact admitted that the wave-theory is fallacious. I am satisfied that there are scientific men who have so much faith in the correctness of the wave-theory

of sound that it would be difficult to convince them that the theory could be incorrect. Their faith might be likened unto the Arabic adage: "If the pitcher fall on the stone, so much the worse for the pitcher; and if the stone fall on the pitcher, so much the worse for the pitcher"—always worse for the pitcher, or for any arguments which attack a theory that has been upheld for 2500 years, and more especially when such theory has been and is sustained by the ablest living scientists.

Let the investigation commence, but do not waste time attacking the less important points; proceed at once to the strong ones, such as the arguments referring to the tuning-fork and to so-called interference, and let the world know whether the wave-theory is the stone or whether it has been shown to be the pitcher by the arguments advanced this evening.